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LECTURES

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BY

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EXTRACTION OF FOREIGN BODIES.

Extraction of foreign bodies continued. Foreign substances in the large intestines. Removal of scybalæ. Bodies in the urinary organs. Effects of balls and shot in the human body. Of pins and needles—remarkable case. Foreign bodies in the trachea. Operation of tracheotomy.

GENTLEMEN,—I mentioned, at the conclusion of my last lecture, that foreign bodies taken into the mouth not unfrequently stop in the rectum; but they may get into the rectum in other ways. Mr. Thomas was sent for to a gentleman under the following circumstances:—He had been very subject to costive bowels, and he used to make them act by introducing a piece of stick or cane eight or ten inches in length into the rectum, and there he left it, until, irritating the mucous membrane of the intestines, they acted, answering the purpose of an injection. He had been in the habit of doing this for some years, but one day the cane slipped out of his hand, and, to use his own expression, "it was sucked up into the gut." At first he was ashamed to send for Mr. Thomas, but after it had been there some days such was the torture that he sent for him in great distress. Mr. Thomas introduced his finger into the rectum, but he could feel nothing. The sphincter muscle gradually relaxed, and he was then able to get in two fingers, and in a few minutes he passed in his whole hand. He then felt the piece of cane sticking obliquely at the upper part of the gut, and he abstracted it without any mischief. There is, in this case, a circumstance of great interest, and one that I believe was first observed by Mr. Thomas, namely, that the sphincter muscle gradually became relaxed under the pressure of the hand, so as to admit not only one finger, but two, and ultimately the whole hand. I have observed the same thing in several cases in which I have had occasion to make an examination, and the knowledge of this fact is very useful indeed on certain occasions which occur not in only hospital, but not unfrequently in private practice. I am very glad to have an opportunity of explaining to you the cases to which I allude, because I remember well that when I first met with them in private practice they puzzled me very much, and I shall be glad if you are saved that perplexity which I suffered myself. Persons of the affluent classes, for the most part, attend a great deal to the state of their bowels, and it is necessary that they should all do so. Those who live luxuriant and indolent lives are liable to have their bowels become very torpid, and you may be assured that there is no harm in their constantly attending to their bowels. I have known people belonging to the affluent classes who have been in the habit of taking medicine almost every day. I know one hearty old gentleman, eighty-six years of age, who can walk round the Regent's-park, who has taken an aloetic pill every night for threescore years. I knew another gentleman, who died at ninety-two, who took either an aloetic or a rhubarb pill for the same length of time, and I could give many other examples. But there are others who do not attend to their bowels; scybalæ form in the colon, they pass on to the rectum, but they are not easily discharged per anum. The softer fæces pass over the scybalæ, other scybalæ descend into the rectum, and the accumulation goes on until at last the rectum becomes completely filled up with a great mass of hardened fæces, as large as the fist, and even larger, so that half a pound or perhaps a pound weight may be collected there. The patient now suffers exceedingly, and he—or perhaps I ought to say she, for it is more common in women than in men—has a desire to go to the water-closet. She goes, great pain is produced, but nothing comes away, the bowels being stopped up with these hardened fæces. The nature of the complaint may be ascertained by introducing the finger into

the rectum; you there feel the hard mass of fæces. How is that to be got rid of? By injection? An injection will not act on this large mass. You must first dilate the sphincter muscle by introducing the fingers, and then with the handle of one or two pretty large spoons the whole mass may be extracted. A good nurse can accomplish it very well, if you tell her how. Let her take a couple of dessert-spoons and bring away a little and a little more, and when the rectum is nearly empty, warm water injected two or three times will remove the remainder. Until I was aware how much the sphincter muscle might be dilated I found it difficult to manage these cases. I used to try to accomplish it by introducing a narrow spoon into the rectum and bringing away a little at a time, but that was a very tedious process.

Foreign bodies may find their way into the urinary organs, and actually into the bladder. There is in the museum of the hospital a preparation of a calculus, which I purchased at the sale of the late Mr. Heaviside's museum. It is a section of a calculus formed upon a hazel-nut. It was extracted from the body of a woman by operation, and on cutting through the calculus the hazel-nut was found in the centre. There was no history of the case, but it is evident that the woman, playing some foolish trick with herself, had forced the hazel-nut through the urethra into the bladder. Mr. Thomas gives an account of a case in which he extracted a silver toothpick from the bladder. A woman had some difficulty in making water —probably an hysterical difficulty—she introduced the toothpick into the urethra, and it slipped back into the bladder. Sir A. Cooper cut a woman for what was supposed to be stone in the bladder, and when he removed it, it was found to be a piece of coal which had been thrust up the urethra.

No doubt these things are generally done from that peculiar perversion of mind which you find in very hysterical women; but it is sometimes done as a mere cheat, for the purpose of exciting compassion, and obtaining money from compassionate persons. A woman at Ryde, in the Isle of Wight, consulted Mr. Bloxam, a gentleman educated at this hospital, and who now resides there, for stone in the bladder. He introduced a pair of forceps and removed a stone of a very peculiar kind. By and by she had another, and he removed that. He thought they were very odd-looking stones, and as I happened to be at the Isle of Wight he showed them me, and told me that the woman had then got a third. We examined them, and they were evidently pieces of common lime-stone, that the woman had cut into such a shape that she could push them into the bladder. She found it a good trade, inasmuch as she obtained money from the compassionate ladies of Ryde an account of her sufferings. I brought the stones up to town, and Dr. Prout examined them. Here the stones were really passed into the bladder; but I may take this opportunity of stating, by way of guarding you against what occurs in private practice, and, indeed, in hospital practice, that very frequently people pretend to pass calculi from the bladder which were never there at all. It is often very difficult to understand what motive there can be in women for trying to deceive in this respect. We can only attribute it to that perverted state of mind which I mentioned before, and which frequently amounts to insanity. Mr. Childer long ago—for he has now been dead twenty years—brought me a wafer-box full of what were said to be calculi passed from a young lady's bladder. On looking at them I said, "Calculi! they are bits of brick-bat and flint, and nothing else." He replied, "It is true, but there is a singular history belonging to them." He then told me this story:—A young lady, the daughter of a gentleman of fortune, all at once began to bleed, and, as she said, passed these calculi from the bladder. Her father and mother went to stay at the house of a country gentleman, and there she was taken very ill indeed at the watercloset, discharged a great quantity of blood, and produced an immense quantity of calculi, which she said came from the bladder; but they were examined very carefully, and found to be just what I have stated. I might mention many circumstances of the same kind. Among poorer people it is sometimes done for the sake of exciting compassion. A woman produced her little boy who was said to pass stones from the bladder. They were sent to me to examine, and I found that they were nothing but pebbles and flint. It was evidently a trick to get money from compassionate ladies, and in which she was successful.

But foreign substances find their way into the urinary organs of the male as well as the female. A man came here with symptoms of stone, but on passing the sound the stone was felt anterior to the bladder. Sir Everard Home cut him for the stone, and brought out one that was narrow, but two or three inches in length. On making a section it was found to be formed on a flower-stalk. The history of the case was this:—The man was a gardener in the country; he had a stricture of the urethra, there was difficulty in making water, and occasionally he used to pass a flower-stalk as a bougie and relieve himself. One day the flower-stalk broke, it remained in, and formed the nucleus of a stone, half in the bladder and half in the urethra. I operated on a young man for stone in the bladder, and on cutting through the stone there was a large piece of common wax in the centre. The preparation, I believe, is in the museum. This was a very foolish young man, as you may suppose, who happened unluckily for himself, to have a wide

urethra, and in some fit of folly he rolled up a piece of wax, introduced it into the urethra, and it gradually found its way back to the bladder. I saw him at the time, and, as I supposed that the wax had gone into the bladder, I recommended him to keep quiet, and let the case be thoroughly investigated. But he was engaged to go to India; he did not suffer inconvenience, as if from the wax in the bladder, though we had a right to conclude it was there, and, contrary to my advice to keep himself quiet, he sailed for India. He came back two years afterwards with a stone in his bladder. A more extraordinary case occurred in the practice of Mr. Keate: I saw the patient with him, and assisted in the operation. A gentleman had symptoms of stone in the bladder, and on cutting into that organ he found that there was no stone, but a great piece of common sealing-wax, of which he drew out several inches in length. This monstrous blockhead—for so I must call him—being tipsy, thought he would pass a bougie for himself. He imagined that wax was wanted for a bougie; he therefore procured the sealing-wax, softened it by the fire, rolled it up in his hands into the shape of a bougie, introduced twelve or thirteen inches through the urethra into the bladder, and there it lay coiled up.

Foreign bodies may get into other parts of the human frame. A musket ball, for instance, may lodge in it for many years, doing no harm. A gentleman of my acquaintance was wounded the day before the battle of Waterloo. There was the hole at which it entered, but none at which it appeared to have escaped, so that it was no doubt lodged within. After a time the wound healed, and he got well. He was a young man, he frequented balls, danced like other people, and felt no inconvenience from the ball. He died several years afterwards of disease of the brain. But at other times musket-balls lodging in the human body may do great mischief. In the museum is a section of a diseased elbow which was amputated here. On sawing through it longitudinally a musket-ball was found in the centre of the bone, which had produced the disease of the joint. But musket-balls, even when lodged in soft parts, do harm in another way. A gentleman was shot in the eyes with small shot, and it produced the most dreadful case of neuralgia that I ever met with. I presume that the shot pressed on the optic nerve. A gentleman had a musket-ball lodge in the leg. It could not be felt, and as it gave him no inconvenience it might be doubted whether it was there or not. By and by it shifted its place, and became more superficial so as to be felt under the skin. Spasm now occurred in the leg, followed by fits resembling those of epilepsy, and to these he was subjected while the ball remained in this position. After a time the ball again shifted its place, went back, so that it could not be perceived externally, and then there was an end of the fits and of the other nervous symptoms. I presume that when it first shifted its place it pressed upon some nerve, and produced the spasm and these fits. Unfortunately, when it was in this situation, and might have been extracted, that course was not pursued; and when it again receded it would have been in vain to attempt it. It was of no use to look for a ball that you could not feel externally.

It is not uncommon for pins and needles to be found lying in the cellular membrane. Sir Charles Bell describes the case of a woman who had an abscess in her chest, from which was extracted a pin some two or three inches in length. It was supposed that the pin had been swallowed, and had made its way out through the œsophagus into the cellular membrane. In other cases it has been supposed that a pin or a needle which has been swallowed, has worked its way through the œsophagus into the chest or neck. But cases sometimes occur in which needles are taken out of human bodies in large numbers. The following case occurred in my practice:—A lady of hysterical habit was unfortunately married to a gentleman who became insane. Once or twice during a paroxysm he very nearly murdered her. What with anxiety about him, and apprehension about herself, her nervous system, which was bad enough to commence with, became much shaken. He died, but she remained in a frightful state,—very weak in health, with constant nervous pain on one side, and subject to what are called fainting fits; in fact, a sort of hysterical catalepsy,—that kind of fit which is produced by animal magnetism working on the imagination of hysterical women, in which the patient appears to be unconscious, but is not so in reality. One day she had with her a paper of needles, containing about fifty, fresh from the place where they were bought. She was by herself, she rang the bell in haste for the servant, and said that she had had one of her fits, and that the needles had run into her leg. This seemed a very odd story. Only eight needles out of the fifty were found left in the paper. It was thought that they had got into the footstool. That was unpicked, but nothing was found in it, except a few broken pins. They looked at her leg, and seeing something they did not understand, they sent for a surgeon in the neighbourhood, who found one or two needles pricking under the skin; he opened the skin with a lancet and took them out. In the course of two or three days other needles were discovered, he tried to take them out, but they slipped away, and I was sent for. With some trouble I removed them, and on a subsequent occasion I took out more; altogether we removed about twenty-eight needles from her leg—they were in one leg only. The leg became swollen and œdematous; and, having been in weak health before, she

now became still weaker, and sank and died apparently from the mere want of nervous energy. On examining the leg I found several needles still left in it; they were not all taken out, but it would appear that there were just enough to account for those missing.

There are two points in this case to be considered; first, the taking out of the needles which, as a practical question, is of some importance; and, secondly, how the needles got there.

It may appear a very simple thing to extract needles that are stuck in a woman's leg, but it is not so simple in practice; for every motion of the limb makes the needles shift their situation; and if, in trying to remove them, you make any pressure upon them before you seize them with the forceps, they slip away. No attempt should be made to take needles out of the human body until they are close to the surface, and when you can with a light hand feel one end of them under the skin. You may then venture to puncture the skin with a lancet, and take care to pass, if possible, by the side of the needle, so as not to make pressure upon it. When you see the black point of the needle take hold of it with forceps and extract it. With a light hand you may take out a needle; but if a surgeon be rough the needle slips away, and extraction is impossible.

But how was it that the needles in the case in question entered the leg. There is only one way of explaining it, namely, that she run them in herself. It is ridiculous to suppose that a paper of needles could run in by themselves. In this state of hysterical catalepsy the patients are not insensible. You know how the girls who are magnetised deceive and cheat. They pretend to read with the back of their head, and prophecy all sorts of stuff, and it is just the same here. This woman was humbugging herself in one way as they do in another. I have no doubt that she run the needles into her leg herself. I can conceive that one needle or two may run in by accident; you may sit down on some needles, and one or two may enter without your being aware of it at the time, but that a whole paper of needles could thus run in I do not believe. When a boy, I read in the "Annual Register" an account of an extraordinary case of a young woman who had swallowed a quantity of needles. The circumstance was forgotten, but years afterwards the needles made their appearance, and they were extracted, some from the arm, some from the breast, and some from other parts. This story was gravely recorded as one of needles having been swallowed, and then finding their way out of the stomach into different parts of the body many years afterwards. If a quantity of needles passed into the stomach I should think that they were more likely to do mischief to that organ itself, or to the intestines and peritoneum, than to run separately, and find their way out at the arms and legs. But I cannot understand how a woman could swallow twenty needles. Could you swallow twenty or thirty fish-bones? Certainly not. We know that hysterical women cheat in all manner of ways, and I have no doubt that these women run the needles in themselves. I do not, however, advise you, when called in, to expose such persons; for that is neither a kind nor a right thing to do. I have before said that some of the very best disposed young women will, when under the influence of hysterical disease, play tricks of this description. One young lady who, I believe, when in health, was as good and honest as she could be, puzzled several medical men for a long time by mixing ink with her urine; and there are a number of stories of the same sort.

Foreign bodies may find their way into trachea, and I shall conclude this lecture with a few observations on that subject.

A foreign body generally finds its way into the trachea in the following manner:—The patient has something in his mouth, he tries to speak just in the act of swallowing it, and in the effort to speak the epiglottis is raised just at the time when it ought to be shut down, and the morsel gets into the glottis. If it be large enough to stop the glottis it produces suffocation. It may, however, occasion coughing, and the cough generally brings it up, but at other times, instead of being coughed up, it slips down; that is, although the patient coughs, yet it slips down; and in other instances it slips down before he coughs,—and then you have a foreign body in the trachea.

The foreign bodies that thus enter the trachea may be very numerous and very various; for example, cherry-stones, almonds, pieces of meat, pieces of bone, gold and silver coin; and the effect they produce differs according to a variety of circumstances, according to their shape, and their particular position. The foreign body may be so large that it descends to the bifurcation of the trachea, and will not go down farther. It may be so large that it nearly fills up the diameter of the trachea, but that is not often the case, for a body that is small enough to go through the glottis will seldom be of sufficient size to fill up the trachea. Besides that, if it be broad it seldom lays directly flat across, but obliquely, and then there is a space on each side. Again, it may be very light, so that it rises up at every attempt to cough; or it may be very ponderous, so that it remains always at the most depending part. It may be small enough to pass down into one of the subdivisions of the trachea, and if it do it generally passes into the right bronchus, because that is the wider, and lies more nearly in a line with the trachea than the left. Supposing the body to be light, such as a cherry-stone or an almond,

and smooth, and being smooth, moveable, it may lie at the bottom of the trachea, and the patient experience no inconvenience from it until a fit of coughing is excited. Several instances have occurred in which a small foreign body has been coughed up again through the glottis; and in other cases, being raised by the act of coughing, it has stuck in the glottis, strangled the patient, and produced instant death. But supposing that, without being very ponderous, it is of a large irregular shape, with sharp edges, and is lying across the trachea, with the corners stuck in one part of that passage, or of the bronchus; then it does not occasion at first much difficulty of breathing; for very probably there is sufficient space for the air to pass by its sides. But, being in the trachea, it brings on inflammation in the mucous membrane, attended with a great secretion of mucus; and this viscid mucus stops up that part of the opening of the trachea which is not blocked up by the foreign body, so that the tube becomes completely obstructed, and the patient dies of suffocation. This is another way in which it may prove fatal.

But supposing the foreign body to be composed of metal; that being a heavy substance, it will keep at the bottom of the trachea if it be of large size, or if small it will descend into the bronchus. Generally speaking it passes into the latter situation; for a metal body that is small enough to go through the aperture of the glottis will usually be small enough to be carried by its own weight to the bottom of the bronchus.

Another question arises here. What will happen when a ponderous body—a coin or other metallic substance—lodges in the bronchus? It will not cause great difficulty of breathing; for generally it is small enough not completely to obstruct that bronchus, or, at any rate, the patient can breathe with the other. It may occasion coughing, and if the patient invert himself so as to bring the head downward and elevate the chest, it will run down to the glottis and threaten suffocation. But the patient is in no danger of suffocation if he do not put himself into a position which, not being a natural one, it is not likely that he will do. It may, therefore, remain lodged there for a long time; but what will happen at last? It will give rise to disease of the lungs. A man swallowed a Louis-d'or, which got into the trachea; he died three or four years afterwards, and on examination there was found disease in the lung, the Louis-d'or being in an abscess. This case is recorded in the "Memoirs of the French Academy of Surgery." A boy got a sixpence into the trachea; he was sent to Guy's Hospital, where it was found that his lungs were diseased, and therefore Mr. Key refused to make any attempt to remove the sixpence. A boy swallowed a tin tack; when I saw him he was expectorating pus, so that it had formed an abscess, and it was doubtful whether the tin tack were there or not. By and by he coughed up the tin tack, and the discharge of pus ceased, and the patient recovered.

Foreign bodies are very frequently coughed up, and it has been proposed to endeavour to expel them by making the patient cough. No doubt you can make the patient cough, and there is a chance of the foreign body being thus removed; certainly, if it is to be got rid of by natural efforts it must be by coughing. Some have proposed to bring up the foreign body by giving the patient an emetic, but I apprehend that it would be useless. In the act of vomiting there is a deep inspiration, not a forcible expiration; the diaphragm descends in order to press on the stomach, and, combining with the action of the abdominal muscles, it expels the contents of that organ. After vomiting is over there is no convulsive coughing, so that the diaphragm gradually returns to its own place. The idea of exhibiting an emetic is founded altogether on a wrong physiological notion; and I think it is dangerous to trust to the act of coughing for bringing up the foreign body. It is very true that it may be coughed up through the glottis; but it may stick in the glottis, and then the patient would die.

Now, what is the best rule to follow? If you are satisfied that the foreign body is in the trachea, I believe that the proper course to pursue is not to trust to nature; she may manage it, but you are not certain of it, and in a great number of cases where it is left to nature the patient dies. Make an opening in the trachea, and I believe that it is best to make it low down. You may proceed here as leisurely as you please, for the patient is not in danger of instant suffocation. Take up every vessel as you proceed, and separate the parts as much as you can with a director, instead of cutting them, so as to avoid hæmorrhage. If you open the trachea when bleeding is going on, every time the patient inspires blood is drawn into the trachea, and the patient may be suffocated by the surgeon opening the trachea too hastily, and allowing it to become filled with blood. I know a case in which a surgeon performed tracheotomy, and the patient died almost directly. On examining the body, as I am informed, the trachea and bronchi were full of blood. Make the opening, then, as leisurely as you please; separate the parts by a blunt instrument rather than a knife; divide three or four rings of the trachea longitudinally, but there is no occasion to remove any portion of the trachea. What will now occur? When you have divided the trachea, if it be a light and moveable body, such as a cherry or tamarind-stone, as soon as you have made the opening, if you hold back the edges, cough comes on, the foreign body is thrown up, and escapes by the artificial opening; or even if it do not escape there

the danger of suffocation, in consequence of its sticking in the glottis, is prevented. But if the foreign body be a bone or any rough substance that is stuck in the trachea, and not moveable, then you may introduce the forceps and remove it; and I can conceive of cases in which it is right to take a foreign body from the bronchus. I advise you, however, never to attempt the latter if you can effect the object in any other way. The introduction of forceps into the bronchus will occasion violent coughing, great irritation, and it is a frightful thing to introduce these instruments into the bronchus when the patient is agitated by a convulsive cough Only conceive of the important organs in the neighbourhood. The lungs are below, and you may injure them, or you may take hold of one of the subdivisions of the bronchi instead of the foreign body. There is the pulmonic plexus of nerves behind; you are close to the phrenic nerves; you are not far from the great vessels of the heart; and the heart itself is close by. Think of the mischief you may do by poking among these important organs with forceps when the patient is agitated by a convulsive cough. Still, if there be a piece of bone stuck across the bronchus, it may be the only way of taking it out. But it does not often happen that any ragged body will go into the bronchus; if, however, it do, you may have to introduce the forceps five or six inches from the part where you make the wound externally.

But suppose a case in which there is a loose and ponderous body in the bronchus. In the case of Mr. Brunel, which occurred last year, there was a half-sovereign in the right bronchus. This gentleman, in playing with a child, flung a half-sovereign into his mouth, and it slipped down the windpipe. In the first instance it produced sickness, and as he drew his breath, previously to vomiting, it descended into the bronchus and occasioned coughing every now and then. When his head was placed down it could be felt rolling along the trachea. We attempted to remove it by placing him on a moveable platform, so that his feet were up and his head down nearly at right angles. The half-sovereign descended and stuck in the glottis so as nearly to choke him. We, therefore, determined not to repeat this experiment till we had got an opening in the trachea which would act as a safety-valve. We made an opening some few days afterwards below the thyroid gland, but the half-sovereign was not coughed up as a cherry-stone would have been, because it was too heavy. We made some attempts to use the forceps, but found it so dangerous that we desisted. When he had recovered from the effects of this operation,—in the mean time passing a probe every now and then,—we again placed him on a moveable platform, his back was struck with the hand, and the half-sovereign escaped from the bronchus. He could feel it rolling along the trachea till it came to the glottis, and now, instead of sticking there, it passed through, just as you could roll it through the dead body, and came out of the mouth. There was no spasm of the glottis, and the absence of it was to be attributed to the opening in the trachea; for blood came out with the half-sovereign which had evidently passed in from the external wound, and where blood went in you may be sure that the air went in also. I apprehend the rule to be this:—In all cases where a foreign body has got into the trachea you must not trust to nature, but make an opening into the trachea; and then it is very likely that if the body be light it will be forced through the opening; or if, by its own weight, it can be made to assume a certain position, it will pass out through the glottis; or, if it be a rough, irregular substance, and sticks in the trachea, you may then, through the artificial opening, seize it with the forceps and extract it. But I advise you to be very careful how you use the forceps, except where the foreign body is actually in the trachea; cases may occur in which you must use them in the bronchus, but it must be done with the greatest possible caution.

ON THE EFFECTS OF

CHLORIDE OF ZINC,

AND

CHLORIDE OF LEAD,

IN THE TREATMENT OF

CANCER.

By E. W. Tuson, F.R.S., Surgeon to the Middlesex Hospital.

During the progress of my investigation of cancerous diseases my attention has been directed in many cases to the effects of certain preparations of chlorine, either in arresting the progress of this malignant disorder, or having a powerful influence over it: and this has induced me to employ various combinations of chlorine in their treatment. The results (from time to time) will be brought before the public, and although the use of a paste made with chloride of zinc and flour, as an external application in an ulcerated state of cancer, is well known, I am induced to believe the profession are not so well acquainted with the administration of this remedy internally, and also with the effect of chloride of lead, and that the following cases may prove interesting to some of the readers of The Lancet.

Case I.

Anne Scammel, ætat. 39, a cook, was admitted into the Middlesex Hospital, under my care Nov. 12, 1839, with an extensive can-

cerous disease of the right breast and neck, occupying the space between the lower border of the inferior maxillary bone and the under part of the pectoralis major, and from the sternum internally to the whole of the superior extremity on the outer side, so that the disease involved nearly one-sixth part of the body. She stated that the glands in the armpit first became enlarged, and she fancied the swelling arose from cold, about three years previously; that it had gradually increased since, affecting at first the breast, producing great tumefaction, and afterwards the whole arm, which became so much swollen that she could scarcely use it or bring it to her side. An irregular ulcer extended from below the nipple towards the axilla, its surface being superficial, and its edges hard. The whole of the parts enlarged were very firm; pressure with the finger on the arm or back of the hand produced little or no indentation. The glands below the jaw and above the clavicle were greatly enlarged, and the general hardness in the neck was so considerable that the clavicle could only be distinguished by a very diligent examination. The treatment consisted of dressing the ulcerated surface with the white precipitate ointment, and she was ordered to take a pill three times a day, composed of five grains of extract of hemlock and one of opium, and a night-draught of tincture of opium in camphor mixture. This plan was continued with little variation for about two months, without any beneficial effects either to the ulcer or the disease, which increased, and appeared of a very malignant character. The swelling of the arm continued, notwithstanding the use of flannel rollers. In January, 1840, a paste was directed to be applied to the ulcerated surface, made of one part of chloride of zinc, and three of flour; this was well mixed and moistened with water, and then applied over the whole of the ulcerated parts. The first application was ineffectual, so that it was used again, when also it was resolved to give the chloride of zinc internally. Half a grain was ordered every morning in a wineglassful of carraway water to be taken after breakfast. A lotion was directed to be applied to all the hardened parts, made of one drachm of chloride of zinc to a pint of water. The application of the paste a second time produced great pain for four or five hours, when a slough was formed, which separated with the assistance of a poultice, and afterwards the ulcerated surface healed kindly. The swelling in the arm gradually subsided, the breast diminished considerably, and that side of the neck assumed a more natural appearance. In a month from this time the case had completely changed its character. The arm was now of its natural size; the breast was soft, as also the neck, and every part of the superior extremity. In several places there were small tubercles, some of the size of a pea, others a little larger; some were irregular, others smooth and circumscribed: there were six or eight at different places over the mammary gland, one or two in the neck, and one in the arm between the deltoid muscle and pectoralis major; they appeared to be enlarged lymphatic glands. The disease had now changed its character, and assumed the cancerous tuberculated form. The wound had completely healed. The medicine was continued for upwards of two months, the dose being increased to three-quarters of a grain. She complained of the draught being very unpleasant, tasting like metal, and leaving a roughness in the mouth for some time afterwards; one morning she took it before her breakfast, when it made her very ill and uncomfortable, causing severe spasmodic pains in the stomach; she was desired never to take the draught on an empty stomach, but to continue it regularly after breakfast. At the early part of April further improvement had taken place in this patient; the disease had been allayed to a considerable extent; she suffered no pain, felt in good health, and all tokens of disease had disappeared with the exception of the little spots. The arm, the breast, and that side of the neck, were in the same natural state as on the opposite side; she slept well, enjoyed her diet, walked daily out in the garden, and when any inquiry was made as to the state of her health she expressed herself confident that in another month she would be able to leave the hospital quite cured. Few people who had seen her at her admission would have believed she was the same patient, such a change having been effected by the medicine. This favourable change lasted only a short time; at the end of April the mouth and gums became sore, so much so, as to cause the medicine to be discontinued. Soon afterwards the arm began to swell again, and the breast became tender and painful. An ulcer appeared at another part of the mammary gland, which was healed by the application of the paste, when other ulcers were formed, which also healed by the same means, until at last the paste lost its influence, and the disease made rapid progress in spite of all remedies. Morphine and opium gave her a few hours sleep, but the cancer progressed, and she died on the 7th of Jan., 1841.

Post-mortem Examination.

A section being made of the mammary gland, it was hard and pear-like. The integuments were involved in the cancerous disease, which also extended to the axillary glands. The cervical glands intersecting the muscles beneath were considerably diseased. The ribs were very brittle, and a finger pressed upon the anterior part of any one readily fractured it.

Thorax.—On the surface of the pleura pulmonalis there were numerous tubercles of a cancerous character, as also on the

pleura costalis. The viscera were healthy, with the exception of the heart, which was soft and flabby.

Abdomen.—All the viscera were tolerably healthy, with the exception of the liver. Upon its surface there were many tubercles, some larger at its posterior border. On cutting into this organ there were several, evidently of a cancerous nature, situated deeply in its glandular structure.

Remarks.—The swelling and hardness, occupying so great an extent, might have been produced by an effusion of albumen or some deposit peculiar to this disease in the cellular tissue, which, being taken up by the lymphatics (excited by the influence of the chloride of zinc) left only the various tubercles, which also diminished in size by absorption. The cells must have been considerably extended, and filled to their utmost, so as to prevent any pitting. Had the cancerous disease existed to such an extent, the change that took place could not, it is presumed, have been accomplished.

This case illustrates the peculiar effect of the chloride of zinc in this cancerous disease, its influence upon the system, affecting the mouth and gums; and further observation may show some other effect of this preparation. It cannot be doubted that its external use is beneficial in healing open cancer, which I have succeeded in doing four or five times successively in several cases; and this should induce us to watch its effects, administered internally, in diseases of this character. A solution of one drachm to a pint of water injected upon a cancerous ulcer, or applied with linen rags, will be found a very useful application. Other preparations of chlorine have an influence over cancerous affections—chloride of lime; chloride of lead; terchloride of carbon; perchloride of copper, &c.; and a case has been described to me of a lady completely healing a large open cancer by the application of common salt (chloride of sodium). From my observations on the effects produced by the exhibition of various combinations of chloride, I am persuaded they have a very powerful influence over cancerous diseases; and I fully intend to investigate this subject, to combine two or more of these preparations, as well as to employ other new chlorides (which I have ready) internally; for it must be confessed by all conversant with the nature of these diseases, and with the pathological appearances seen in most cases, that they can only be counteracted by affecting the system, and thus acting on the constitution generally, and not by local application alone. Should, hereafter, a cure be found out for these inveterate diseases, it must be by internal agents, counteracting some peculiarity in the system, permitting certain parts to be affected by this disease, or some morbid defect in the nervous system, so that the natural and healthy structure of the component parts of the animal texture are no longer preserved, but sink into this peculiar state of disease. The nature of cancer and its structure generally has been fully studied during a long period, and at present little useful knowledge as regards the treatment of the disease has resulted from the investigation, either by entering into its first formation, growth, or chemical composition. In forty cases I have noticed a trifling point (which may be fallacious, however); inquiry was made in respect to the diet of the patients, and whether they were in the habit of eating salt meat, or taking much salt at their meals, and the reply was (with the exception of one lady) that they disliked salt meat, and took very little salt. This led me to investigate the nature of the blood in several cases, to ascertain whether there were any chemical deficiency in the saline agents of this fluid, and on one occasion I thought some useful conclusion might be adduced; but upon a further and more elaborate investigation my hopes were blighted in a great measure on this point. The following case is interesting, inasmuch as it will exemplify the beneficial effect of another preparation of chlorine in cancerous affections:—

Case II.

Mary Bradley, ætat. 42, was admitted into the Middlesex Hospital under my care Feb. 14, 1843, with a large, irregular, open cancer of the right breast, extending deeply into the axilla, so that the pulsation of the vessels could be seen, with a foul, yellow, excavated surface, copious discharge, with much fetor and severe pain. She stated that two years and nine months previously Sir A. Cooper had removed a scirrhous tumour: after the operation the wound nearly healed, but broke out again into an ulcerated state a fortnight afterwards; only the scirrhous swelling was removed. The rest of the breast with the nipple was left.

Feb. 15.—(*Extract from note-book.*)—The ulcerated surface extends deeply in the axilla down to the vessels, and over a large surface, from the centre of the sternum, to and below the axilla; it presents a very unhealthy appearance, with dark, excavated spots at various parts, the fetor very great, and the discharge considerable. The following lotion was ordered to be applied with linen rags constantly kept wet:—

℞ *Chloride of lead*, one drachm;
Water, a pint.

17. The surface of the cancer has a more healthy granulated appearance, more so in some parts than in others; the pain, she states, is less at the lower part, but much the same at the upper.

21. The surface appears more healthy; the granulations are red, and the edges of the ulcer have the appearance of healing; the

pain is less. The following draught to be taken three times a day:—

℞ *Chloride of potassium*, ten grains, in carraway-water.

24. The whole surface has much improved; complains of very queer sensations in the head, with loss of memory, and a dulness all over the head and eyes. Omit the draught.

March 13. The whole surface of the ulcer has become much better, and the edges are cicatrising slowly; she suffers less pain, and the fetor has ceased.

17. The edges of the ulcerated surface have a very healthy appearance, and at some parts have healed to the extent of an inch; but those parts that have healed appear to be only covered by a thin transparent membrane, unlike the healing of any other wound. In parts about the centre there are unhealthy excavated spots, which have much the appearance of the wound at her admission; she says she is pretty free from pain, and sleeps well at night.

21. The wound is still healing; the part healed is smooth and shining; it has the appearance of the ulcerated part being covered with a portion of goldbeater's skin; this is probably cuticle. The irregular excavated spots are still more unhealthy than when last examined. The axilla has considerably filled up since her admission, and the edges are healing in the same way as the other parts of the wound. She suffers little pain, but has been complaining of griping pain in the stomach and bowels. She was ordered an aromatic draught; she also complains of cough, for which she has been taking the compound squill pill. The discharge is less, and free from fetor. When the dressing is removed there is a blackness and discoloration upon the skin, caused by the action of the lead, of the same appearance as seen upon the edges of a stump after amputation. She was ordered to continue the lotion, but to be watched on account of the pain in the bowels.

24. The ulcerated surface remains about the same; the spots have increased in size, and appear to approach the edges of the wound; the part that has healed appears healthy. She has been pretty free from pain, and the sensations felt in the stomach and bowels have ceased; towards the axilla a great improvement has taken place; it has healed considerably, and has filled up nearly to the surface of the skin.

28. The ulcer in the centre has much the same appearance; the edges are not so well; small ulcerated spots, of the size of a pin's-head, have been formed; they seem to extend deep, and the thin membrane at parts has become absorbed. She was ordered to take *liq. chlorini*, ℥i; *water*, ℥iss, three times a day.

31. The ulceration is about the same, with the exception of the edges; the small ulcers have increased, and some have acquired the size of a small pea; they are deep, and contain a thick, white discharge. The lotion of chloride of lead to be discontinued, and the following one used:—

℞ *Chloride of zinc*, ℥j;
Water, ℥x. Make a lotion.

She said the medicine did not agree with her. It was ordered to be discontinued.

April 4. She has been using the lotion continually since our last visit; the ulcerated surface has the same appearance. The application caused no pain nor inconvenience.

7. Remains much the same; perhaps at parts the ulcerated surface has a more healthy appearance. The lotion to be increased to double the former strength.

11. The ulcerated parts have a more healthy appearance at some places, and the edges are better; the spots have diminished in size and depth. The lotion still causes no pain; she says she remains much about the same.

15. The cancerous surface presents a cleaner appearance, with less discharge; she complains that the lotion gives her pain now when it is applied, and that this lasts for some time. She was ordered to discontinue it, and to use the former lotion.

21. The whole surface of the ulcerated part remains much about the same; the spots are increasing, appearing dark and unhealthy; the edges of the wound are becoming more irregular.

28. The whole appearance of the ulcer remains unchanged.

May 5. The ulcerated surface appears a degree better; she suffers little pain, and sleeps well at night; she complains of being very low, and was ordered some port wine.

12. The dark spots have sloughed at some places, and the wound has been bleeding. The muriated tincture of iron has been applied.

19. The wound appears cleaner; no bleeding since our last report; she remains about the same.

26. The wound is more unhealthy; the spots at the edges have again made their appearance; they are deep, and discharge a thick pus.

June 2. The ulcerated surface has been bleeding several times; the former application was employed on each occasion with success; she is very low, and complains of her chest and cough. Comp. squill draught three times a day.

9. The whole surface has a more unhealthy appearance, and altogether the patient is worse in every respect; she is very low and restless; bleeding not so frequent; She was ordered wine and a little brandy with an egg; to go on with the draught.

16. The ulcerated surface appears more unhealthy; it has sloughed at small places, and bled a little several times; the wound has

extended deeply into the axilla. From this time the patient gradually got worse, had great difficulty in breathing, no doubt from effusion of serum in the cavity of the thorax; the ulcerated surface bled several times, the muriated tincture of iron always stopped it; and she died on the 30th of June, having been in the hospital four months and a fortnight. No post-mortem examination was allowed to be made.

Remarks.—In this case the effects produced by the chloride of lead and the effort of nature to assist in the removal of the disease by the ulcer filling up and healing at the edges, were most striking. The disease having again an ascendency over the system, destroying the edges at several parts, the patient being relieved from pain by the application, and life prolonged, were remarkable. Little doubt can be entertained that had not the application been used bleeding would have taken place sooner from the ulcerated surface near the axilla, which would have been fatal at the time; or she would have died from exhaustion, as the vessels at her admission were distinctly seen in the armpits. The chloride of lead has been very extensively used in many cases of disease of the breast and uterus in the form both of lotion and ointment, with much success; it appears to act upon the nerves, paralysing them, and thus producing ease. One young patient used an ointment made with one drachm of the chloride of lead to an ounce of common cerate, rubbing it over the breast and armpit; it produced a complete numbness of the arm, and relieved the pain: the ointment was discontinued, and the numbness subsided in the arm: it was still used over the breast, and the patient was cured. This ointment will be found of most use in hysterical affections of the breast, or nervous irritability of its gland, and also in other cases, particularly when any tumour presses on or involves a nervous filament, and thus produces constant pain; the ointment applied upon the course of the nerve, over the situation of the pain, will give ease; it is also of much use in allaying inflammation, and it forms another valuable remedy in the treatment of these diseases.

Russell-place, Fitzroy-square,
Jan. 1, 1844.

Treatment of Chancre.—M. Ricord's favourite application is lint impregnated with aromatic wine. This is a decoction of sage, rosemary, thyme, hyssop, mint, &c. in eight times their weight of Burgundy wine. It destroys the contagious quality of the secretion from a chancre and *tans* the neighbouring parts, rendering them less susceptible. M. Ricord has now and then found benefit from the addition of from one to six per cent. of tannin to the foregoing, to promote the latter purpose.—*Prov. Jour.*

AN

ANALYTICAL INQUIRY

INTO THE NATURE OF

THE PROCESSES OF DIGESTION AND NUTRITION.

WITH PRACTICAL DEDUCTIONS.

By George Ross, Esq., Kennington.*

General Remarks.

It has been the custom among physiologists to attribute the formation of certain products to certain organs, simply because they have been found in connection. For example, urea was once supposed to have been formed in the kidneys, and choleic acid is still supposed to be a product of the liver; in the former case, indeed, the opinion has been abandoned, but in the latter the dogma, although resting upon a foundation equally rotten, continues to be supported. If it should prove correct, it will not receive its justification from past evidence or modes of reasoning.

Again, erroneous views of the functions of organs have obtained from the same cause. Thus, the mouth has been styled the organ of mastication; the stomach the organ of chymification; the duodenum the organ of chylification; yet, when these distinctions acquired importance, no physiologist was able to inform us upon what ground he established them; or, if correct, in these local ascriptions, he would have been perplexed to tell us the mode in which these organs performed their functions. Now, we are not justified, in strict reasoning, to say that any structure is the organ of a certain function unless we can prove it to be so, either by positive fact or negative evidence. If I, at the outset of these papers, were to pronounce the mouth to be a *stomach*, I should probably be declared mad, yet I should be quite as rational as those who call the stomach a *stomach*—by which is meant the organ of chymification. Further, the duodenum has been termed the organ of chylification, and yet acute physiologists have doubted whether the chyle may not be really included in the chyme of the stomach. Each of these organs may be the receptacle in which certain changes take place, but neither of them is, to the full extent, the cause of these changes, as is implied by the ordinary nomenclature. An organ must, necessarily, be the cause of the changes ascribed to it, but as yet we do not know that the duodenum forms chyle any more than that the bladder secretes urine, or is instrumental in the changes that take place in the urine while deposited within its cavity. These expressions, it may be said,

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* Author of "The Diagnosis (Chemical), Pathology, and Treatment of Typhus Fever," Lancet, vol. i., 1842-3, pages 782, 809, 853, 885.—Ed. L.

are mere solecisms, adopted for the sake of convenience; but I apprehend that much hindrance to science has resulted from them on account of their tendency to confine the attention of the inquirer to a particular situation, where, perhaps, the truth he seeks may not be discoverable.

A knowledge of the digestive process is the most important a medical practitioner can acquire, not only on account of its immediate relation to the support of the system, but also because, as a sanatory and remedial means, he has it always at his command. Yet, perhaps, there is no medical aid so much neglected. A patient is too often advised, with much energy, "to take nothing that does not agree with his stomach," because the practitioner has not sufficiently studied the laws of digestion to be able to render such a special opinion as a few moments' reflection might have supplied. But perhaps this remark will fall with less force, upon a consideration of the obscurity which still hangs between the observer and the secrets of nature.

The Proximate Principles of our Food.

Our food is derived both from the animal and vegetable world. Vegetable food consists chiefly of sugar, gum, starch, oil, gluten, &c., none of which substances, with the exception of gluten and a few others, not named, contain nitrogen. Of the non-azotised vegetable products, starch is the most abundant of those used as food by man, since we observe that it forms 96 per cent. of rice, the chief article of consumption among native Indians; 73 per cent. of wheat, the staple of our own diet; that it is the basis of the potato and various other tubers, and exists pure in arrow-root, sago, cassava, sowans, &c., all important articles of food in the various countries in which they are produced. Sugar is obtained from the sugar-cane, beet-root, and other roots, and from the various fruits. This is also a highly nutritious substance. Gum, or mucilage, is a chief constituent of the green parts of vegetables, and is abundant in linseed, with which horses and cattle are soon brought into condition; but in this substance there is also a considerable quantity of oil.

It is remarkable, and appears to be a general law, that all nutritious substances in the two respective classes of azotised and non-azotised principles, should have a similar atomic constitution. The equivalents of the three chief vegetable proximate principles are thus expressed,—

		C	H	O		C	H	O
	Sugar ..	12	11	11	or	10	10	10
	Arabin ..	12	11	11				
Starch	Amylin ..	12	10	10				
	Amadin .	10	10	10				

We perceive that the difference in the composition of these principles is so small, as to be of little account in our consideration of the changes produced on them by the digestive process. This difference is evidently dependent upon the various amount of the equivalents of water entering into their composition; for if we add one equivalent of water to sugar and arabin respectively, and two equivalents of water to amylin, we obtain the same atomic composition for them all. I apprehend, therefore, that there is no essential difference in their atomic proportions.

Sugar the Ultimate Principle of the Digestion of Vegetable Food.

The similarity of their composition naturally suggests the probability of their conversion into some form mutual to all, and this probability we find to be founded in truth. Gum and starch are equally convertible into sugar by the action of dilute acid; hence sugar may be considered as the radical or generic principle of these non-azotised vegetable substances. The occurrence of these changes, however, under the influence of ordinary chemical action, does not establish the fact of the same transformations taking place during the process of digestion. Yet we find that the same law holds good under both circumstances; and it is probably as convincing a proof as we possess of the chemical nature of the digestive operations. If the stomach be examined during the digestion of starch, we perceive that this principle is converted into gum of starch and sugar; and it seems probable that gum is the intermediate stage of these actions.

Saliva, the Digestive Fluid for Vegetable Principles.

This point, therefore, having been established, it became necessary to ascertain the agent of these changes. It was seen by the experiments of Schwann, who submitted various substances to the action of pepsin and dilute acid together, and separately, that starch did not, under any of these circumstances, undergo the changes observed in the digestive process. Hence neither pepsin, dilute muriatic, nor acetic acid, were sufficient for the digestion of starch; the conclusion naturally follows that the stomach is not the organ instrumental in this process. Further research has, however, informed us that if starch be mixed with saliva the usual changes are effected, and this result appears to be due to the salivary matter. Saliva, when fresh, is nearly always alkaline, consequently the changes produced on starch cannot be attributed to free acid; unless, indeed, any free sulphuric acid should be evolved during the process,—a point which is probable, and requires further examination. However this may be, the due admixture of saliva is absolutely necessary for the digestion of starch, a most important point to establish in physiological investigation, since

starch forms so large a proportion of our aliment. I have now brought evidence to prove the proposition I advanced at the outset, namely, that the mouth is as much a *stomach* or digestive organ as the stomach itself; in fact, its glands appear to be the only structures essentially necessary for the assimilation of one great division of nutrient substances.

Deductions.

We desire our patients, as a general rule, to take sufficient time in the mastication of their food, in order that the operations of the stomach may be assisted, and the rule is good. But it is manifest that the mere division of the food by the teeth is a mechanical process subordinate to some superior object, and that this object is not simply to prepare the food for the action of the gastric-juice, but also to saturate it with a sufficiency of saliva, by which the necessary changes dependent on the presence of this fluid may be accomplished. The office commonly attributed to the saliva of mixing with the food, and forming it into a globular mass, fit for deglutition, is only a secondary one, and has no reference to the peculiar chemical composition of this secretion.

The chief requisite for the digestion of starch is, that it should be thoroughly imbued with saliva. To attain this end the substance taken as food should be of that dry and porous character, that it may unite with a considerable quantity of the fluid; and, also, that the time required for mastication may be as long as possible. I need scarcely instance the fact of the digestibility of stale bread, or of dry biscuit, which, combining the required properties in a still higher degree, is proportionably more nutritious. A given weight of dry biscuit will sustain the body much longer than the same amount of bread.

If farinaceous food, taken in the solid form, be already so much impregnated with water that it requires but slight mastication to acquire the necessary form for deglutition it will be more or less innutritious, since the saliva cannot have become intimately mixed with the mass. On this account the whole family of puddings have suffered proscription. A heavy pudding is an abomination, which no human stomach can tolerate. It coheres in the stomach into a solid ball, and, although it should become diffused, yet the flour, as I have witnessed, is not digested even after twenty-four hours; and it never can be, on account of the absence of saliva, and diarrhœa or constipation is the necessary consequence. For the foregoing reasons, nothing is more injurious than to take a large quantity of farina, and, to facilitate deglutition, wash down each mouthful with a copious supply of liquid. A much larger quantity of matter is thus taken than is necessary to repair the waste of the system, and the intestines become burdened with the undigested mass.

Starch is often taken in a liquid form, and arrow-root, sago, tapioca, &c., are commonly thus prepared. Such preparations are called "slops," to indicate that they yield but little nutrition. They are, in fact, mere diluents, and the starch appears to me in these cases to be converted chiefly into water and lactic acid. When I have taken arrow-root, prepared with water, at night, for the sake of observation, I have found that the desire to pass my urine became extreme about five or six o'clock in the morning, but that after my urinary organs had been relieved further irritability and pressure did not supervene. There had been an increased secretion of urine on account of an increased formation of water, but there had not been an excess of a stimulating agent. My object in making these observations on starch in the liquid form, is to guard the practitioner against falling into the error of administering farina in this manner, with a view to its nutritious properties. It is not sufficient to give a certain amount of starch to obtain a specific degree of nutrition, but it must be also given in the mode by which it may be submitted to the action of the saliva, and undergo a nutritive digestion. I have seen individuals condemned to live upon starch actually die for want of support, and the attendants, meanwhile, dreaming that they ought to be sufficiently supported, since they have taken such an immoderate quantity of food. I remember more than one lamentable instance of this fact. Bad nursing has sacrificed many a hecatomb to ignorance.

Liebig, it is well known, denies any nutritive power to starch, believing that its ultimate end is to maintain respiration. I think there is reason to doubt this position; but, if it be true, the patient is still in a more dangerous predicament. The sagacity with which Liebig has prosecuted this department of physiology makes one careful of doubting any of his propositions; and although science may not be allowed to stand still with Liebig, yet the world owes him all deference and gratitude. The medical public of England is also much indebted to Mr. Ancell for his able and lucid exposition of Liebig's doctrines.

History of Opinions concerning Lactic Acid.

I have observed that starch is, probably, under certain conditions, converted into lactic acid, and since there is considerable difference of opinion respecting the origin and relations of this principle, I will now enter into a short disquisition on this point. Liebig has stated that the existence of lactic acid is, in all cases, dependent upon decomposition, and, consequently, that it is not a proper constituent of the gastric-juice. MM. Leuret and Lassaigne, on the contrary, affirmed that the acidity of the gastric-juice

was entirely attributable to the presence of lactic acid. Professors Tiedemann and Gmelin do not appear to have discovered lactic acid in the stomach, but refer its presumed existence to the presence of acetic acid. Dr. Prout allows it to be a secretion from the stomach, but only under circumstances of disorder of the digestive functions, produced by disease or indigestible aliment. He also believes that this principle may be derived from the aliment itself. Thus the four highest modern authorities on digestion are at issue, and leave us only in doubt. I may remark, however, that opinion strongly inclines to the side of the Germans, and that the exclusive principle maintained by MM. Leuret and Lassaigne has been long since exploded by Dr. Prout.

Pathological Observations on Lactic Acid.

Chevreuil found lactic acid in the fluid vomited by a patient *fasting*. Dr. Graves twice observed it in the matter vomited by dyspeptic patients. Dr. Watts, of Nottingham, states, in a paper on diabetes, some time since published in THE LANCET,* that he has discovered it in the watery portion of the alvine evacuations of a diabetic patient. In the case of a lady of weak digestive organs, and who had been attacked with sickness and diarrhœa twenty-four hours after having partaken of some rich pudding, and which, at the end of this period had remained undigested, I found lactic acid abundantly present both in the fluid vomited and in that passed by the bowels. *The matter vomited was neutral.* In another case of a lady who, in consequence of chronic inflammation of the intestines, experienced great irritability of the stomach, and towards the commencement of her recovery was afflicted with diurnal sickness, generally in the afternoon and while fasting, I did not observe lactic acid. I must remark that the fluid was slightly alkalescent. At a later period, however, I was informed that the brash had become sour in taste, and now contained the remnants of her food; it was, therefore, submitted to a second examination, and lactic acid was found. These cases, therefore, demonstrate the fact of the production of lactic acid from indigestible aliment, but render a doubt as to its being a secretion from the stomach. Chevreuil observed it, indeed, in the fluid vomited by a person fasting; but this is a vague expression, for although the individual may have fasted for a considerable period, yet that does not preclude the probability of the products of the last digestion still remaining in the stomach.

Experiments proving Lactic Acid to be a Product of Decomposition.

On account of lactic acid being abundantly found in rennet, it has been supposed that it exists in the calf's stomach in the natural state of this organ; but this is not correct. I procured the stomach of a calf that had been killed fasting, so far as concerned its natural diet. On examining this organ, a quantity of the bed of its stall, consisting of shreds of straw and hair, was found in a semi-digested state. In the most digested portions these substances were much comminuted, and formed a pultaceous mass, surrounded with a layer of mucus from the stomach.

This mass may be considered indigestible in reference to the stomach of a calf that had been hitherto fed upon milk; it did *not*, however, contain lactic acid, nor was there any found on the inner surface of the stomach after a short maceration in water or acetic acid. Muriatic acid was, however, perceived in abundance.

After the inner surface of the stomach had been macerated in water twenty-four hours, lactic acid was discoverable in the liquid. A similar portion of the inner surface macerated in acetic acid during the same period did not evince the least portion of lactic acid. Hence I concluded that acetic acid, by arresting decomposition, prevented the formation of lactic acid. If, however, the fluid were kept until decomposition commenced, lactic acid appeared in the acetous solution.

When the chymous mass was thus treated with water and acetic acid there was no appearance of lactic acid. This mass, composed of straw and hair, did not readily undergo decomposition.

After the inner surface had been exposed to air twenty-four hours, and then macerated in water, it immediately betrayed the presence of lactic acid, which was again clearly the effect of decomposition. When, however, it was macerated in acetic acid, lactic acid was not discoverable.

The aqueous solution of dry rennet, when examined within an hour, contained much muriatic acid; as the decomposition increased, the lactic acid increased in proportion.

When the dried inner surface was digested in alcohol and examined early, there was a larger proportion of muriatic than lactic acid. After twenty-four hours there was an increase in the proportion of lactic acid.

Further, I observed that the *outer* surface, after maceration in water, yielded lactic acid; consequently, this principle is not peculiar to the inner surface, but is the simple result of decomposition.

These experiments prove that lactic acid is not secreted in a healthy state of the stomach of the calf, and that even comparatively indigestible substances will not cause its secretion. A question here arises as to whether it is ever secreted, or whether, when discovered, it is not always either the result of decomposition of the food or of the

* Vol. ii., 1842-3, p. 65.

mucus accumulating on the stomach in consequence of a weak digestive apparatus? For myself, I entirely subscribe to Liebig's opinion, and believe that lactic acid is, in all cases, the result of decomposition. I apprehend, also, that when lactic acid is found in the alimentary canal it is a product of the decomposition of our food. Future research, I have no doubt, will make this a valuable means of diagnosis in disorders of the stomach and bowels.

Lactic Acid produced by the Decomposition of Sugar.

Gelatine, albumen, and saccharine substances, whilst undergoing decomposition, evince lactic acid; but it is more especially from the last that this acid is obtained. Liebig observes that an ordinary animal bladder, when exposed to air and moisture, and placed in a solution of a sugar of milk, effects a change to lactic acid in this substance; also the lining membrane of a calf's stomach, if first dried in air and then placed in sugar, changes it into lactic acid, or mannite and mucilage, or alcohol and carbonic acid, according to the stage of decomposition. This effect is not peculiar to these substances: caseine, after precipitation by pepsin, will convert sugar into lactic acid, as I shall hereafter detail.

Liquid lactic acid is a syrupy substance, and its composition is identical with that of sugar, consisting alike of equal atoms of carbon, hydrogen, and oxygen. The transition, therefore, from one substance to another, may take place without the exertion of much chemical action; and the formation of lactic acid would appear to be the first stage of the decompositions taking place in the digestive fermentation of sugar. I apprehend, indeed, that the formation of lactic acid is in the peculiarity of the digestive fermentation; but more of this hereafter.

Observations on Acetic Acid.

These observations on lactic acid lead me to offer a few remarks on acetic acid, which it nearly resembles in composition and qualities. My attention having been drawn to a foot-note in Thomson's "Vegetable Chemistry," which observes that "it is curious to remark that starch-maker's water, and the still sourer water of sowans, are swallowed greedily by pigs, and they fatten upon it,"—I made inquiries as to this fact, and found that starch-maker's water was largely employed in London for feeding hogs. This "refuse water," as it is termed, contains the starch that is not precipitated with some acid. Through the kindness of Mr. Coward, one of the proprietors of a large starch manufactory at Lambeth, I was supplied with some of the "sour water," or the menstruum from which the starch is formed, and which contains more acid than the "refuse water." The specimen given to me contained 3.60 per cent. of acetic acid. Sowans is made by fermentation from the husks of oats, when starch is precipitated with the formation of an acid, which is, I presume, the acetic.

Use of Acetic Acid in the Digestion of Starch.

It is the property of an acid to cause a large flow of saliva, and I have no doubt that it increases the secretion of this fluid from the glands of the pig, in which animal it is generally abundant. By this means the starch undergoes a more perfect digestion—that kind of nutritive digestion which capacitates it for the formation of fat,—a subject that will occupy our attention on the next occasion.

Deduction.

Any substance that will act as a sialagogue will promote the digestion of farinaceous substances; a reason is thus given for the large employment of piperaceous and aromatic substances by Indians, which has, hitherto, been a problem in physiology. They live chiefly on starch, and instinctive feeling directs the use of these condiments, by which the conditions of a nutritive digestion are secured. In England lemon-juice is constantly employed with liquid farinaceous food, in order to make it sapid,—in other words, to increase the flow of saliva, and thus facilitate digestion. We, likewise, always mix salt with our bread for the same purpose. I have no doubt that if a small quantity of acid were added to the meal with which swine are fattened, this process would be much accelerated.

(To be continued.)

RUPTURE OF THE HEART.—M. Peste has recently reported to the Royal Academy of Medicine the following case:—A man, 77 years of age, of sober habits, had, about eighteen months previously, been attacked by apoplexy, which left him paralysed on the left side. For the last eight months he had been an inmate of the Bicêtre. On the 17th of June, after a hearty meal, he was seized with vomiting, and from that time precordial pains and anxiety never left him: he died suddenly on the 19th. On opening the chest, the pericardium was seen to be greatly distended; and on cutting into this, the heart was found buried in a dense clot of blood. On the anterior face of the left ventricle, about its centre, was a transverse rupture, about half an inch in length, and bifid at one extremity. A thin false membrane floated around this opening. The heart was much encumbered with fat. The left coronary artery, at its chief point of sub-division, was dilated into an aneurismal sac, but throughout its whole course it was enlarged to quite the size of the brachial artery, and in several parts it was ossified. The rupture appeared to have been provoked by the vomiting two days before death.

TRIAL OF COLD WATER

AS A REMEDY FOR SUPPOSED DISEASE.

DEATH.

POST-MORTEM APPEARANCES.

To the Editor of THE LANCET.

SIR,—The following case of what literally is cold-water murder having recently come under my observation, I beg to send its history to you, for insertion in THE LANCET.

Mr. C., ætat. 52, a highly respectable druggist, residing in the market-place of this town, having slight headach, but not suffering under any serious indisposition, was induced—by reading several popular treatises on hydropathy, which he perused with great interest, and in the statements of which he placed a blind and credulous faith—to avail himself of the advice therein contained, and to commence the treatment recommended by them. Previously to the commencement of this Mr. C. had been accustomed to take one glass of ale at dinner and one at supper, with an occasional glass of wine once or twice a week, but was a man of great regularity, very domestic, and of spare frame. During the last days of August he commenced the cold-water plan of treatment (discontinuing all fermented drinks,) by swallowing, during the day, from seven to eight pints of cold water, sponging night and morning, and taking a considerable amount of exercise. This system was continued until the commencement of December, when Mr. C. began to find himself becoming thinner and weaker; that the quantity of urine passed was disproportionately great to even the large quantity of water taken; that the tongue became red and dry, the thirst excessive, attended with a dry state of skin. From this time it is impossible to ascertain the quantity of water drunk, as Mr. C.'s thirst induced him to take it in greater quantities and drink more frequently than he had previously done.

This state of affairs continued until Thursday, the 21st of December, when, for the first time, he sought a medical opinion, and called in his usual medical attendant, Mr. Hawksley, a surgeon of very extensive practice in this town, who found him with the following symptoms:—Exhaustion very great; much emaciation; pulse small, feeble, and fluttering, beating seventy-two in the minute; skin cold and dry; the thirst and appetite very great; lower extremities cold; urine in great quantity, he having passed for some time from five to six quarts during the twenty-four hours, limpid, insipid to the taste, reaction on test-paper acid, specific gravity 10.25. Notwithstanding the frequent exhibition of stimulants, these symptoms continued with little or no improvement until the Wednesday following, when the stomach became very irritable, and vomiting commenced, solids as well as liquids being rejected. I was then desired to visit him, and found him in the state described above. An opiate was prescribed; effervescing medicine, with hydrocyanic acid, and the tincture of sesquichloride of iron, to be taken as frequently as the state of the stomach would permit; beef-tea, brandy, with soda-water, and small portions of solid food. Mr. C. passed a good night and appeared better during the next day; but towards the evening the vomiting returned with increased frequency, and continued during the greater part of the night, until completely relieved by the frequent administration of one drop of creosote. Nevertheless, the exhaustion increased although stimuli were used very freely, both internally and externally; these had the effect of rousing him for a brief period, but every effort to rally him for any continued time proved unsuccessful; he gradually sank, and died at half-past one o'clock, p.m., on Friday.

The Autopsy was made early in the morning of 31st December, by Mr. Sibson, in the presence of Dr. Percy, of Birmingham, Mr. Hawksley, and myself.

The brain presented no appearance of disease, was tolerably firm and consistent Viscera of the chest: heart pale and flabby very slight pleuritic adhesions on left side; lungs generally œdematous. No effusion into the thoracic cavity; the viscera presented generally a pale anæmiated appearance. The mucous membrane of the stomach injected from the effects of the vomiting; mucous membrane of the whole intestinal tract softened. The kidneys rather more deep in colour than usual, soft in texture, and, on their section, presenting numerous small bloody points. The urinary bladder empty and contracted. Other viscera in a healthy condition. No abnormal effusion.

The preceding case presents an example of the effects of large quantities of cold water, long continued, being taken into the system. It is quite clear that the constant call upon the action of the kidneys, from the quantity of liquid taken, eventually brought on a super-excitement of their natural functions, and induced diabetes, from the exhausting effects of which upon the general constitution the patient sank. The examination of the body proved the existence of no organic disease. The softening of the mucous membrane of the intestinal canal may justly be attributed to the debilitating effects wrought upon the blood from its extreme dilution by the quantity of liquid taken. This respected gentleman, this father of a family, this valuable member of society, might, as the examination proves, have lived to the usual age of man (or perhaps beyond it, for his mother is now living at the advanced age of eighty-seven), but for

the unfortunate monomania which beset him, and which made him one of the victims of this now prevailing and rapidly-increasing folly of the present day, the cold-water mania.

It is little creditable to the boasted intelligence of our countrymen, that Great Britain should, in the nineteenth century, be still acknowledged as the El Dorado of quacks, the paradise of charlatans. Let any proposition, however absurd,—perhaps the more irrational it is the greater credence it obtains,—emanate from any part of the world, designing and desperate men immediately appropriate it here, make it a source of profit to themselves, unscrupulous of the injury they may be inflicting on others, but knowing they have a rich mine to work in the gullibility of a certain portion of the public, which must bring to themselves that which alone they covet—gold.

A few years have seen the rise and fall of Saint John Long's inhalations and embrocations, of Morison's pills, of mesmerism, of homœopathy, of the brandy-and salt mania, and of numerous other quackeries. All of these have had innumerable advocates, and many, too, of them many victims. Their diminished heads are now all hidden, however, under a sheet of water,—that which has been christened by the name of "hydropathy," and well named too, for, literally translated, this term means *water-disease*. Hydropathy is now the panacea for all ills, the remedy for every disease, and, moreover, the exclusive one, none being required but water—only water. It might be supposed that all diseases were composed of nothing but *fire*, which could alone be destroyed or extinguished by water. After a few years it may with safety be prophesied that the existence of a folly so extreme as this will scarcely be believed unless some of the hydropathic establishments which, on a larger or a smaller scale, exist in most of our more important towns, should remain as monuments of a bygone and almost incredible infatuation. But if it is to be lamented that a quackery so baneful to health, so dangerous, from the extreme manner in which it is applied, as "hydropathy" (I admire the name), should receive credence with a certain portion of the public, what terms of indignation can be found sufficiently strong to apply to its advocates who, to the disgrace of those universities where they obtained their degrees, still continue members of our honourable profession? In what words shall we speak of those medical men who, pandering, for the sake of filthy gain, to this credulous spirit, publish, in every manner, plausible and sophistical arguments, and pretended facts, with the sole object of bringing to themselves *gain*, though to *others* loss of health, if not of life.

The press is, in every sense of the word, inundated with their publications; the subject of the preceding case owes his death solely to the reading of these, with a too apt belief that they "*must* be true," emanating, apparently, from respectable physicians or surgeons, or, at least, from those who have the assurance, the unblushing assurance, to appear as such by appending the letters "M. D.," &c. &c., to their names. The majority of these hydropathic doctors being well educated, they cannot exculpate their conduct by throwing over themselves the shield of ignorance. They are not fools, but wise enough in their generation and in worldly cunning. Not being fools then, I will leave others to decide what substantive will best express their proper appellation. The respectable members of our profession have long looked with mingled pity and sorrow upon the suffering victims to this water-drenching treatment. They have acted upon the *populus vult decipi decipiatur* principle, and have waited, expecting that the water-bubble would burst, and that the whole system would gradually be immured in that contemptuous tomb of all the Capulets in which so many of its predecessors have been interred. I have participated in those feelings, and have refrained from making any observations to destroy that which has appeared to be so likely to be its own annihilator if peaceably left to itself; yet the case I have related is so striking in its course, and so fearful in its termination, that I have considered it a duty incumbent upon me to give it publicity. I have the honour to remain, Sir, your very obedient servant,

R. S. Hutchinson, M.D., M.R.C.S.
Senior Physician to the General Hospital, near Nottingham.

Nottingham, Jan. 1, 1844.

CASES IN PRIVATE PRACTICE.

EPILEPSY.

TWO ATTEMPTS TO COMMIT SUICIDE.

EMPLOYMENT OF SULPHATE OF ZINC.

By William Ryan, Esq., Surgeon, Sutton Coldfield.

History of the Case previous to my attendance.—Thomas Spencer, æt. 50, attacked by epilepsy for the first time thirteen years since; was previously, his wife says, as stout a man as any in the country, in the flower of health, and never complained of headach. He had, at this time, nine distinct fits, and afterwards "slept" for a week, during which time he was given a little nourishment, by force. When raised for that purpose he cried, like a child, but never had the use of his faculties. At the end of the week he awoke, and said it was time to go to work, inquiring why they allowed him to overlie himself so long, thinking he had

gone to bed on the previous night. During the first three days of the attack he spat out whatever was given him. Had fits all the first night, every two hours, from twelve o'clock, the time of his first attack. He got up after the third fit, acting outrageously; snatched his watch, and would have broken it, and endeavoured to tear his clothes off. He came down stairs, where he had the remaining fits. "His eyes shone like silver; he looked wild." He remains dogged and speaks surlily a day or two before the attack, but knows nothing of its approach until the last moment, when he is struck down, making a muttering kind of noise, and thus generally remains in the fit three or four days. He will not believe a fit to be coming on, but if warned of it, feels annoyed, and asks why they trouble him by their nonsense. I cannot find that it was inherited, none of his family being subject to it. He was employed in making roads about thirteen years since, *and was then affected by severe relaxation of the bowels* during three or four days, accompanied by delirium. *After this he always appeared poorly for three or four months, when the first attack came on.* Before the first attack his wife noticed that he had great difficulty of breathing in his sleep, the act of respiration taking greater time than usual, and accompanied by a loud (sonorous) noise. Some time before the fits come on he complains of headach,; his feet cold as ice; on touching his head with his hat he observed it felt hollow, like a drum, and as if a noise were in it. Feels pain going up the right side of head, and is much troubled by flatulence, being often obliged therefrom to leave his work, unbutton his small-clothes, and hold them so on his way home. Some time before and after the fits he is very irritable, and no one can do anything to please him. Passes urine during the fits, and once, after the second or third fit, passed fæces. When he gets thoroughly out of one fit, another comes on, two hours generally intervening, but the interval depends on the time elapsing between the first and second; thus, if an hour's remission take place, the subsequent fits observe the same time. Has sometimes got off with one fit, and continued an hour or more before perfect consciousness returned. "*When the ninth fit comes on he gets as black in the face as possible*, and then his looks are awful indeed." His eyes look very dim, as if he were dead, and this is certainly much more observable after *a ninth fit* than at any other time. After having many fits he lies almost insensible for three days. When spoken to and offered anything he pushes it off, merely asking why they trouble him. During this time, if left to himself, he never speaks; seems dozing, although not in a regular sleep. Days before the fits a semicircle appears, "white as snow," above the eyebrows, his ears also being white. His wife then informs him of her opinion, but he will not hear of it. He answers questions till the last moment preceding the fit. No aura. He used to bite his tongue during the fits, but has "grated" all his teeth out during the attacks. *He has twice attempted to hang himself.* Once, six or seven years since, went into the hovel and fastened the door; when his wife got in she found his neckerchief laid aside, his shirt-collar unbuttoned, the rope fixed to a joist and a noose on the other end, ready to put round his neck. His wife expostulated, and he burst into tears. He looked frightfully pale. He obtained a rope a second time, but was, luckily, observed. This first attack he was bled in the temple and arm to a great extent, *and had another fit at the stopping of the blood.* Was bled again during the week. The fits, from this period, came on every six or eight weeks, for twelve months, when they disappeared for some time, and then began again with another bad attack, repeated, as before, every six or eight weeks, *he being bled during each attack to about a pint.*

Remarks and Treatment.—Such is the history of this interestiug case up to the period of my attending him for the first time in March, 1839. He was sitting up when I saw him, maintaining a dogged silence, and scarcely answered my questions by a monosyllable. The expression of his face was peculiarly sullen. You felt, having asked him a question, doubtful whether he might or might not strike you; until the monotonous tone of voice being heard, and the passive fcatures observed, a person felt convinced that the fierce look was not assumed, but that the poor patient was stamped by the "peculiar physiognomy" of the disease. He seemed anxious to avoid conversation. Sallow countenance; pulse slow, very weak and small, and stopped by the smallest compression. The whole appearance of the patient convinced me that whether or not there might, at times, be cerebral vascular action, the case was, in fact, one of asthenic epilepsy, with deficient nervous influence, and derangement of the digestive organs. I felt satisfied that to adopt a course of *depletion* would be to aggravate the disease, and therefore resolved on a fair trial of *tonics*, and amongst the first, that which I had before found of great service, when used with discrimination, sulphate of zinc. On hearing his wife attribute the complaint to great exertion in raising a weight, followed by a severe attack of the bowels, we are reminded of the remark of Frank and others, that epilepsy seldom occurs in persons previously perfectly healthy; and we also observe that material fact, so judiciously observed by Copland, that injudicious bleeding causes a return, when the stream is stopped, of the paroxysm—which fact, when occurring under the care of a practitioner, should prove a valuable index to his future

treatment. *It should be also particularly borne in mind that it was after the last bleeding of the first attack that the man remained soporose for a week.*

I first ordered compound extract of colocynth, a drachm; chloride of mercury, five grains—mix for twelve pills, two to be taken occasionally; and after their operation, sulphate of zinc, twelve grains; distilled water, six ounces—mix, and let two tablespoonfuls be taken three times daily.

Six or eight weeks afterwards he was attacked as usual. The zinc was then increased to three, and, finally, to four grains three or four times a day, a moderate action of the bowels being kept up. A much longer period intervened before the next seizure, and by the end of the year, instead of the usual number of six or eight, he had had but *four* attacks, the last being very severe and consisting of *nine fits*. From that time, until about the 15th of the present November, a period of three years and three-quarters, he was not affected by the complaint. About two months since, he fell from a very high tree, and it is a question of some interest whether such fall might have been a cause of the subsequent attack. In the present seizure he had had four fits, *but weaker than any he had ever previously*. I have again put him on the sulphate of zinc, and, *permissu* SCALPELLI, will at some future time publish the result.

Hereditary Predisposition.—As addenda to the foregoing, and intimately relating to the question of hereditary predisposition to the complaint, I may remark as follows:—Spencer has seven children; one boy, aged seventeen, has fits, sometimes forty in a day; his mother once counted forty-seven. He is not aware of their coming on. When seized he goes *backwards* some yards before he falls. "His heart stops working, and when his heart begins to move he comes to himself that instant." Attacks come on every five or ten minutes, with perfect consciousness between. If standing at the time of the seizure, he never falls without first walking backwards. He is sometimes a day or two without fits. Complains of great headach. Head cut all over with the numerous falls. In falling he broke the clock with his head, and once fell into an open well, said to be eight yards deep. Has been thus attacked seven or eight years. His mother also observes a curious squinting (convulsive), occasionally, in a boy, six years old, and in a child a year and a half old, and fears epilepsy. When the eldest falls he does not seem convulsed, but like one dead, and remains so "until his breath comes;" he then leaps up *in an instant, and goes to any thing he has to do, appearing ashamed of any one seeing him*. Recollects nothing of the fit. This seems to be the "*petit mal*" of Andral. I shall try what can be done. I attended the infant during dentition for convulsions.

INTERESTING CASE OF

CONGENITAL BLINDNESS

TREATED,

SUCCESSFULLY, BY OPERATION,

AT THE

AGE OF EIGHTEEN YEARS.

By Dr. FRANZ.

THE son of a physician residing in London, scrofulous, but otherwise robust and of good powers of understanding, of a family which is predisposed to diseases of the eye, was born with strabismus convergens and cataract of both eyes. An injury and inflammation of the eye occurred to the mother in the eighth month of pregnancy, and she experienced from that time continual anxiety for the health of the child, imagining the offspring might have some share in the disease. Keratonyxis was performed at the end of the second year on the right eye, and was twice attempted on the left, in the ensuing four years. The first operation was followed by violent iritis and partial atrophia bulbi; the second was without any effect. By degrees, however, the turbidity and opacity diminished, and the patient was enabled to distinguish light from darkness (lichtempfindung).

When the patient came to Dr. Franz he presented the following symptoms:—An excessive degree of strabismus convergens; the right eye insensible to light, more immoveable than the left, clearly shrunken and doughy: the left was of its natural form; cornea clear; iris tolerably normal; the pupil drawn downwards and inwards, and insensible; the anterior wall of the capsule of the lens darkened, at many points hypertrophied, and of the appearance of mother-of-pearl.

On looking in a very oblique direction from the temporal side into the pupil, there was perceptible in the anterior wall of the capsule a narrow perpendicular slit, about a line and a quarter (Paris) in length. This little opening was perfectly covered by the pupillar border of the iris; and since, on account of an adherence of its lower border to the uvea, it was held somewhat open, the aqueous humour had free access into the interior of the capsule. The patient had been able to distinguish, with this eye, light, and even colours; and, excepting an occasional feeling of pressure in the eye, there was no pain. The touch of the patient had reached, by long practice, to an extraordinary degree of perfection, especially in the lips, but he could not distinguish colours. The touch of silken stuff gave him great pleasure.

Couching was now performed, the usual antiphlogistic treatment followed, and the case went on well.

On the third day after the operation the

eye was opened for the first time. The patient could see no object distinctly, but imagined he saw a great field of light, with things moving about confusedly in it. The light, however, caused too much pain to allow him to keep his eye open.

Two days later the patient, on opening his eye, saw a multitude of turbid watery globules, which moved with the eye, and, when it rested, seemed to revolve.

Two days after this there were the same appearances, but the globules were less turbid, thin, and motionless, and they seemed more to overlap one another. The patient could perceive a difference in surrounding objects through the globules, but indistinctly; and sight was still painful. The globules became gradually smaller and more transparent, and in the course of a fortnight disappeared altogether.

The patient was much troubled with muscæ volitantes, which appeared as black, immoveable stripes, directed from within upwards, at every opening of the eye. If the eye were shut the patient perceived, especially towards evening, above and externally, an intense blue, violet, and red. These colours became, in course of time, weaker, and passed into orange-yellow and green, disappearing entirely in the course of five weeks. As soon as the patient could look upon an object without pain for a sufficient time Dr. Franz instituted the following experiments:—

1. The patient was shown silken ribands, of the primary colours, and then of the complementary colours, upon a black ground. After a little consideration he learned to distinguish them, except yellow and green, which he often confounded. If they were placed together, however, he distinguished them rightly. Grey pleased him best, because it made a pleasant impression upon his eye. Red, orange, and yellow pained him, but were not in themselves unpleasant. Violet and blue, on the other hand, gave no pain, but were excessively disagreeable, more especially the latter. Black produced subjective colours, and white called forth multitudes of muscæ volitantes.

2. The patient sat with his back towards the light, and kept his eye closed. At some distance from him was placed a sheet of paper, upon which were drawn two thick black lines, one perpendicular, the other horizontal. He opened his eye and named the lines rightly. As he was going to point out the horizontal one he put his finger on the perpendicular one, but soon observed his error. A black square, drawn upon a white ground, within which were drawn a circle and a triangle, he named rightly after a good deal of consideration and delay. He distinguished an undulating from a zigzag line, but could describe them in no other way than by drawing them with his finger in the air. He said that these figures were very strange, he had no notion of them.

3. A globe, placed about three feet from the patient, and on a level with his eye, he looked upon as a disc, a cube as a square surface; he could not distinguish them from similar figures cut out of paper. A pyramid, turned with one side towards his eye, he called a triangular surface. He shut his eye, and the figures were so turned that they presented two sides to him, a larger surface of one being seen than of the other. He declared these figures to be very strange ones, which he could not make out at all. In answer to the question, What sensation had the appearance of these objects produced upon him? he replied, that as soon as he opened his eyes he perceived a difference in the form of the objects, and that they were not figures but solids. However, he had no clear idea of the disc and square until he had a sensation in the ends of his fingers, just as if he actually felt the objects. When, now, he took the solid bodies into his hand he was surprised that he should not have recognised by his sight, objects with which he was already very well acquainted through his touch.

4. In a tall vessel, filled with water, was placed a musket-ball, and on the water a piece of pasteboard of similar colour, size, and form. The patient perceived no difference in the different positions of these objects, both seemed to lie upon the surface of the water, or, since he could not find the bullet upon the water, to lie in the water. When he was informed of the true position he was astonished not to have recognised it by his sight. On going to touch the bullet with a pencil he touched the bottom of the vessel, at some distance from the bullet, and endeavoured then to find it by feeling about. With regard to reflected light, he said that on looking into a mirror he was obliged to recollect that it was fastened to the wall, in order to rectify the apparent position of objects behind it.

When the patient was permitted to use the eye commonly, all objects appeared so near that he sometimes feared coming in contact even with those which were far removed. Everything he saw appeared much larger than he was led to expect by his touch merely. Bodies which moved, particularly living bodies, appeared especially large and near. If he wished to estimate the distance of a body from himself, or from another, without stirring from his place, he moved his head and body right and left, in order to observe the object from different points of view.

Of intricate pictures and of perspective he had no conception,—all things seemed perfectly flat, even when he was acquainted with them by feeling. The patient had no memory for things which he had seen, and even if he had seen an object many times he

could form no mental idea of it, without having it actually before him.

In his dreams, before the operation, he felt only (heard voices). Now, on the other hand, he saw also. The human face pleased him; but of any of the objects that he saw, more especially he admired the eye, particularly when moving. He disliked the nose, on account of its shape and projection, and he was disgusted at the movement of the lower jaw, in eating. He took much pleasure in seeing, but was often tired by the multitude of novelties, of which he could form no clear conception.

The sense of touch long maintained a superiority over that of vision. Two months afterwards Dr. Franz operated for the strabismus. By this the sight of the left eye was improved, and the right was enabled to perceive luminous impressions. Objects were at first seen in places different from their real situations, but this is always observed in those who have been long subject to any great obliquity of the eyes. In the course of two months, however, this disappeared, and by the help of spectacles the patient was enabled to see perfectly well.—*Journal für Chirurgie, und Augenheilkunde*, Drs. V. Walther and V. Ammon, in the *Allgemeines Repertorium*.

*** Two or three points are worthy of notice in the foregoing case, on account of their physiological bearing. First. The patient recognised the pyramid, the cube, &c., to be solids, only after he had had *two* views of them. This accords with Mr. Wheatstone's principle, that two perspective projections of the same body are necessary to our belief in its solidity. Secondly. From this case it appears that, as was supposed by Le Cat, the mind derives great aid in judging of the distance of objects, from having two different aspects presented to it, or, otherwise, from a consciousness of the inclinations of the optic axes. We may account, on either hypothesis, for the patient's judging of the distance of an object by inclining his head from side to side. Thirdly. The patient could not *remember* the difference between yellow and green, although he could *distinguish* those colours. Fourthly. The cause of the colours seen "especially towards evening," immediately after the operation, is obscure. It is remarkable that they appeared in the eye in the same order as those of the spectrum of the sun, when the eye, after being fixed on that body, is turned to a white spot.

In some individuals, from idiosyncracy, the pulse is preternaturally slow; Napoleon's pulse, in health, is said never to have been above forty-five in the minute.

DR. LEE'S UNQUESTIONABLE DISCOVERY AND DEMONSTRATION OF THE ENTIRE NET OF THE NERVES OF THE UTERUS.

To the Editor of THE LANCET.

SIR,—I have been surprised to see the following paragraph, in a note by Dr. Edward Rigby, respecting the nerves of the uterus, at page 21 of his recent edition of "Dr. Hunter's Anatomical Description of the Gravid Uterus:"—"Indeed, Dr. Lee," he says, "appears to express a conscious doubt as to the nervous structure of these masses, for he does not describe them as *continuous* or identical, but as merely *coalescing* with the branches of the hypogastric or spermatic nerves."

I trust your readers will do me the justice to compare this paragraph with the following from my lecture on the nerves of the uterus, published in the "Medical Gazette," Dec. 30, 1842:—

"From the form, colour, general appearance, and vascularity of these plexuses in the body of the uterus, and the resemblance they bear to ganglionic plexuses of nerves, and from their branches actually *anastomosing and coalescing* with the spermatic, hypogastric, and sacral nerves, I was led to conclude, on first discovering them, that they were nervous ganglionic plexuses, and constituted the special nervous system of the uterus enlarged by pregnancy. This dissection was examined by many of the most eminent anatomists in London, and they all admitted that the great plexuses on the body of the uterus were continuous with the spermatic, hypogastric, and spinal nerves. In a paper read to the Royal Society (Dec. 12, 1838), I described the appearances displayed in these dissections and drawings. It was referred, by the Committee of Physiology, to Professor Owen and Mr. Kiernan, and these distinguished anatomists decided, from an examination, with the microscope, of small portions of the plexuses under the peritoneum, which had long been immersed in rectified spirit, that they were bands of elastic tissue, and not plexuses of nerves. The evidence furnished by the actual continuity of the plexuses with the great sympathetic was considered of no weight compared with the microscopic appearances. The microscope was the only test."

It is scarcely necessary to state that this decision with the microscope was afterwards reversed by the Council of the Royal Society, and that it is now universally held to have been erroneous. Mr. Owen now admits, in the most unreserved manner, *that the microscope is not a test*. I remain, Sir, your obedient servant,

ROBERT LEE.

Saville-row, Jan. 1844.

THE LANCET.

London, Saturday, January 13, 1844.

A COMMUNICATION was recently made to the Royal Academy of Medicine, of Paris, by M. NEPPLE, on "The Antagonism of Phthisis and Intermittent Fever," which is certainly the most important and interesting document that has yet appeared on that subject, and we shall take the opportunity, therefore, of giving his letter in full in this place.

M. NEPPLE is a physician, of eminent talent, now practising at Lyons, the author of a very justly esteemed work on intermittent fever, and a corresponding member of the Paris Academy. His letter is as follows:—

During the eleven years that were comprised between the years 1818 and 1829 I practised in the canton of Montluel (Ain*), which is composed of several districts, some of which are marshy, and some quite healthy. I was then much struck by the small proportion of cases of phthisis, scrofula, and goitre, which the marshy localities presented. The frequency of these diseases appeared to decrease as the elements of impaludation became more numerous and more general; so much so, indeed, that in the farms and villages situated in the most impaluded districts I seldom discovered any traces of tuberculisation.

On my mentioning these remarks to a fellow-practitioner, Dr. Dutèche, whose district was much more marshy than mine, he answered that his attention had never been directed to the subject, but that what I said was a ray of light to him, and brought forcibly to his mind the fact that he had never seen any cases of phthisis or scrofula in various parishes which he named, Marlieux, Versaillieux, Lachapelle, St. Nizier, all situated in that part of the country in which the ponds are the most numerous, and the population most debilitated by intermittents. He added that so convinced was he that the atmosphere of this district must exercise a favourable influence on the first stages of phthisis, that he would at once establish a sanatorium for tubercular diseases in an old mansion which had been abandoned by its proprietors on account of its insalubrity. This plan, however, was never realised.

In a letter which I received lately from Dr. Pacond, of Bourg (Ain), I find the following paragraph: — "During forty-five years' practice I do not recollect having seen any cases in the neighbourhood of Montluel which invalidate the results you have arrived at. Formerly my practice extended considerably into the marshy districts, but I have in vain consulted my memory, and various notes which I have by me on the diseases I observed in the marshes; I can find no trace of tubercular diseases. I have not, either, seen a single case of those affections among the numerous patients received into the hospital of Bourg from the marshy country. Not wishing, however, to rely entirely on my own experience, I have consulted those of my fellow-practitioners whose practice is the most extensive and on whose talents I could the most rely. Among the latter is Dr. Hudelet, who has long occupied the post of physician to the Bourg Hospital, and is very often called to the parishes which are in the centre of the marshes, and he does not remember having seen a single case of phthisis in that part of the country. I have, nevertheless, myself remarked that the children of the better class of people who reside in these localities, when placed in schools not under the influence of marshy emanations, lose the benefit of that influence, and sometimes become affected with tubercular disease, in which case the malady is not arrested by their return to their native place."

Dr. Monfrin, physician to the hospital of Châtillon-les-Dombes (Ain, 1843), thus answers my questions: — "Pulmonary phthisis is rare in the district of the Dombes. For the last three years the mayor of Châtillon and myself have noted the cause of all the deaths, and I find that on 400 deaths, comprising those of the hospital, there are only eight by phthisis, one of the eight being a stranger to this country. It is worthy of remark that Châtillon is not situated in the most marshy part of the Dombes district."

MM. Rater and Candy, physicians of some note, attached to the Hôtel Dieu of Lyons, who have practised in the marshy plains of Forez, have also noticed the rarity of phthisis in that locality. Dr. Candy has ascertained that latterly, since many marshes and ponds have been drained, and the wealth of the country has become greater, phthisis has been more frequent.

Dr. Jolly, in an "Inquiry into the State of the Marshes of Arcachon," published

* The department of the Ain is bounded on the east by the Jura mountains, on the west by the Saône, on the south by the Rhone, and is crossed from north to south by the river Ain. It is covered by ponds, lakes, and marshy swamps, and is, perhaps, of all France, the district in which intermittents are the most general, and exercise the most baneful influence on the population. The principal town is Bourg.

some years back, makes similar remarks with regard to the rarity of phthisis in that locality.

With the testimony of so many well-informed medical men before us, we can scarcely doubt the fact of tubercular diseases being much less frequent in marshy districts than in any others. The statistical reports published by MM. Forget and Gintrac are only *apparently* contrary to this fact; in reality, they are not opposed to it. What has, from the first, complicated the question, and rendered its solution difficult, is the want of precision with which it was at first brought forward, and the theoretical propositions which were too soon propounded, and which became, unfortunately, "the subjects" of discussion.

Thus, the great differences which exist in the medical topography of the different localities in which the question has been studied, have been passed over in a much too cursory manner. This being the case, how is it possible for statistics to agree when the elements on which they are founded are so different? MM. Forget and Gintrac have taken for the basis of their observations the inhabitants of the hospitals of Strasbourg and Bordeaux. But the population of these establishments is of a very mixed character, those who compose it having very different habitations, and being placed under the influence of very different hygienic conditions. In these localities not only is the physical state of the soil very different from that of the really marshy districts, but there are also numerous other morbific causes which modify the fevers generated by effluviæ. In giving, therefore, the comparative number of the cases of intermittent fever and of phthisis observed in the hospitals, they have only proved that these two diseases may develop themselves simultaneously in the same localities, and that the causes which are calculated to produce even a great number of periodical fevers are not, of themselves, calculated to preserve from phthisis; in other words, that mere accidental intoxication produced by marshy miasmata does not act as a preservative.

In order for this effect to be produced the miasmata must be sufficiently intense, and continued in their action, to produce marshy cachexia. These conditions are only met with in really impaluded districts, such as the Dombes, and not in those in which the soil is only covered by stagnant water momentarily, accidentally, after great rains or inundations; nor in those in which the marshy miasmata are received from a distance. In such localities great numbers of intermittents may be observed, but the constitution of the inhabitants receives no characteristic modification, as is the case in Dombes, in Sologne, and other essentially marshy countries.

That part of the Dombes in which tubercular affections are, as it were, unknown, is formed by a compact, uniform, clay soil, which is quite impermeable to water, and on which eighteen hundred ponds, of all sizes, have been formed. These ponds are principally formed by rain-water, and communicate with one another, so that they may be emptied and filled at pleasure, according as water is wanted for rearing fish or ground for tillage. Owing to this system of things, a considerable extent of land is alternately covered with stagnant water, or laid dry for cultivation. In the poisoned waters which fill these ponds are generally macerating the remains of the previous harvest, the decomposition of which is indicated by a nauseous odour, *sui generis*, and always percepible in their vicinity. To these ponds must be added innumerable ditches and hollows which constitute so many marshes. The population is homogenous, disseminated in farms and villages, and exclusively engaged in tilling the land and fishing the ponds. Constantly breathing a foggy atmosphere impregnated with miasmata, working in the mud of recently-drained ponds, drinking bad water, and living on food containing but few nutritious and tonic principles, the constitution of the inhabitants has become impressed with peculiar characters. Hippocrates has well described this peculiar state of the organisation which is the result of deep organic debility, of a want of normal irritability of the tissues, of the passive ampliation of the venous system, of the stagnation of the circulating fluids, and of their want of plasticity. This morbid state is always carried to its maximum of intensity by repeated attacks of intermittent fever. It is in the existence of this organisation, to which I would give the name of marshy or aqueous cachexia, that we must look for the physiological reason of the absence of all tendency to the morbid process of tuberculisation, as also for the explication of the doubts of MM. Forget and Gintrac. It is evident that the economy thus modified is no longer susceptible of the same acts, and that all predispositions, as also all morbid repulsions, must present a different character. Every individual whose economy has not become modified, at least to a certain degree, by the influence of this cachexia, will not be safe from tubercular affections. It appears evident to me that the more the elements of impaludation are extended and multiplied, and the more the physical constitution of the indigenous inhabitants is deteriorated, the less frequent do tubercular affections become. This is the fact; I leave it to theorists to apply their systems to it.

Admitting the above fact as demonstrated, can we hope to see phthisis ameliorated or cured by a more or less lengthened residence in marshy countries? To this question I would answer, that there is a great difference

between preserving from a disease and curing it. We can easily understand that certain conditions may prevent the development of certain pathological acts, and yet not be able to destroy the material affection which these acts may have produced.

Nevertheless, although thus expressing my incredulity respecting the curative virtues attributed to the humid and miasmatic atmosphere of marshes, I confess that it does not appear to me impossible that this atmosphere, which, in the Dombes, is not subject to great variations, may be calculated to moderate the pulmonary erethism, and the extreme acceleration of the circulation, and thus to arrest the rapid softening of the tubercles.

Taking this view of the subject, I think that this kind of atmosphere would suit, perfectly, those cases of phthisis in which the heat generated is very great, the cough is dry, often accompanied by sanguinolent expectoration, and where the organisation is very irritable. Intermittent fever is not much to be feared with such persons; besides, phthisical patients bear quinine perfectly well.

After reading to the Academy Dr. NEPPLE'S letter, M. OLLIVIER D'ANGERS remarked that phthisis is equally rare in the lagoons of Venice, that he had visited the hospital of that city and learned from the head-physician, not without surprise, that he had only met with two cases of that disease in a mass of 1200 patients.

AMONG the various societies which have been formed in this country for the promotion of medical knowledge, it is much to be regretted that there is none of which the professed object is to test the efficacy of *new remedies*. Such a body, well constituted, would not only contribute greatly to extend our acquaintance with therapeutic agents, but would afford the most effectual means of checking that worst species of quackery which is carried on within the professional sanctuary, and which, we deeply regret to say, is becoming every day more frequent. A common quack, who professes to be nothing *but* a quack, stands merely in the category of those ingenious persons who derive a livelihood from the gullibility of their neighbours, —a class which, we fear, would prove a very extensive one, if all vocations were subjected to a searching analysis. But a quack within the pale of the profession is the most disgusting of all hypocrites, and the most noxious of impostors. He assumes the honoured insignia of legitimate medicine, while he is secretly endeavouring to enrich himself by the exercise of those despicable arts which he, perhaps, would be the first to declaim against in the instance of the extra-professional charlatan. There are two varieties of "regular" medical quackery, the one of which is based on cold-blooded and unmixed knavery, while the other involves both dishonesty and ignorance. When, for example, a man conversant with the pathology of the lungs, and skilled in the diagnosis of their diseases, puts forth a remedy for the advanced stage of phthisis, and at the same time adduces, in evidence of its efficacy, a set of cases which he well knows are not of the nature alleged, that man presents an example of unmitigated knavery which can scarcely be equalled under any other circumstances. But suppose a man arrayed in all the glories of the medical doctorate, yet extremely ignorant of the pathology of the lungs, and the diagnosis of their maladies—a case, alas, of no unfrequent occurrence. Suppose, further, that such a man stumbles on a remedy which the ignorant or careless observation of a few cases leads him to believe specific in phthisis, and capable of curing the disease even in its advanced stage. His flourish of trumpets in honour of the immortal discovery, might at first seem to be the result of simple ignorance and folly. But if his conduct be viewed more narrowly, it will be found that he also is a knave of a deep dye, though his knavery, being diluted by his ignorance, is of a less diabolical cast than that of the better informed professor of "consumption curable." If a man be ignorant of the diagnosis of a disease, *he must know and feel his ignorance*, whatever parade of science he may make to overawe the credulity of the multitude. How, then, does such a booby as the man in question dare to tamper with so serious a case as that of phthisis? How does he dare to come before the profession and the public with alleged cures of confirmed phthisis, when he knows, in his inmost heart, that he is devoid of the means of discriminating a case of phthisis from any other case?

But if such an institution existed, as that of which we now lament the absence, such enormities as these could never be attempted with impunity. The impostor, whether knave, fool, or both, would at once be detected and exposed, by the fair and simple measure of bringing his allegations to the test of public observation. "Consumption curable," whether by rubbing, or inhaling, or traction, homœopathy, and hydropathy, and all the other "fallacies" and humbugs by which medicine is disgraced, and her legitimate professors tormented, would hide their diminished heads, and quietly sneak out of the arena; and such a trial of every alleged remedy as that here recommended, would afford not only the most efficient, but the only equitable means, of deciding on the pretensions of alleged remedies. However complete may be our conviction of the falsehood of a *new* allegation respecting a matter of fact, we have no *right* to affirm that it is false until we have brought it to the test of direct observation. So far do we carry this principle, that if a man were to profess to us his ability to leap over the monument, we should not say to him as SAMUEL JOHNSON would have done, "Sir, you lie," though we should perfectly know that he was lying; but we should say, "Sir, favour us with an exhibition of the feat." In like manner, when we are told that any substance can cure phthisis in the last stage, we perfectly know that the assertor is either himself deceived, or wishes to deceive us. But we would not arbitrarily arraign the propagator of such an error of ignorance or falsehood. We should merely say, "Allow us to bring before you a certain "number of cases, ascertained, by compe-"tent judges, to be unequivocally phthisis "in an advanced stage, and let us see what "impression your treatment will make upon "them." Let the public be assured that this is the only true way to deal with quackery. Mere condemnation has never yet proved or disproved anything; nor can argument, however grave and well conducted, lead to any more satisfactory issue, so long as *alleged facts remain untested by observation.*

A NOTICE was forwarded to us about three weeks since, by Sir BENJAMIN BRODIE, in which the honourable baronet stated that he objected to the publication of his lectures, and that he had presented similar objections to the Editors of other journals, who had readily consented to act upon his request.

The principles on which THE LANCET has ever been conducted relative to the publication of proceedings occurring within the walls of the "recognised hospitals" of this metropolis, prevented us from acceding to the wishes of Sir BENJAMIN BRODIE without some further investigation into the subject matter of his application. As we considered that there existed, from the constitution of the hospital, and from the nature of the office which we then thought that Sir BENJAMIN occupied in it, a clear and an undoubted right to publish his lectures, we certainly believed that we could not, consistently with our duty, refrain from publishing them, unless the request of the lecturer were sustained by well-defined and clearly-developed public grounds. At an interview which subsequently took place between Sir BENJAMIN and ourselves, those grounds were distinctly presented to our view, disclosing the facts,—first, that the hon. baronet has, for some time, declined to occupy any office in St. George's Hospital;* and, secondly, because it forms no part of his duty to the students of the hospital that he should deliver to them a course of clinical or any other lectures.

These statements on the part of Sir BENJAMIN at once determined the question as to our right to publish. We instantly and unhesitatingly admitted that we had no such right.

An appeal was then made to the hon. baronet on another public ground, and he was asked whether it could be his desire, as the lectures were actually delivered in a public institution, that the benefits derivable

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* In the last prospectus put forth by the conductors of the school of St. George's Hospital, Sir Benjamin was certainly announced as consulting-surgeon to the hospital, and he has been so announced in the heading of the lectures which we have published, and, by an inadvertence, that title has been retained in the present number of THE LANCET.

from their publication should be restricted merely to the gentlemen to whom they were addressed, and whether it could, by any possibility, be his wish that the medical pupils throughout the empire, and young practitioners in particular, should be deprived of the advantages which a perusal of the results of his ample experience could not fail largely to confer.

Sir BENJAMIN explained that his time was very much occupied; that he had not leisure to look over the reports; and that he thought that the publication of "cases" might occasionally give pain to the relatives of afflicted persons. He promised, however, that he would bestow a candid consideration on the question, and forward to us the result of his deliberations. Sir BENJAMIN afterwards communicated to us his decision; and we congratulate students in medicine, and our junior medical brethren, on the result,—as the objection to the publication of the lectures is withdrawn. They will now, therefore, stand recorded in our columns as a free-will offering to the medical public.

The determination of Sir BENJAMIN not to interfere with the publication of his discourses must give additional lustre to his already great reputation. We are at issue with him on the subject of medical politics. The principles which are held by Sir BENJAMIN and ourselves, with reference to medical government, are as wide as the poles asunder, and never can be reconciled; but that circumstance shall not induce us to refrain from stating, that we consider the clinical lectures which he is now delivering to be some of the best and most useful practical discourses that have ever been addressed to a class of students of medicine. There is an absence of effort about them, an avoidance of trick and ornament, a simplicity of style, and a choice of easy and familiar language in the detail of useful facts, which really prove that these lectures emanate from a well-stored, powerful, and gifted mind. This admission is due to Sir BENJAMIN BRODIE. It is made with hearty feelings of satisfaction, and, knowing that it is his due, it would be withheld from our columns with pain.

From what we have stated it must be evident to our readers that the hon. baronet is not answerable for the accuracy of his lectures as they appear in our columns. But we may state that they are written from notes taken by one of the first short-hand writers in London, and that a surgeon attends each lecture, and afterwards revises the proof before it is finally issued from the press.

PAYMENT OF MEDICAL FEES BY LIFE-ASSURANCE COMPANIES.

To the Editor of THE LANCET.

SIR,—I have observed with satisfaction the increasing attention paid by professional men to one of the grievances under which they have long laboured, namely, that of being called upon to give gratuitous opinions to life-assurance offices. It is obvious enough that whatever fees may be paid to us, in future, for services of this kind, they can make but a small item in our receipts, and I trust that the public will perceive that it is the *principle* for which we contend rather than the emolument. Indeed, whether we consider the party who is laying by money for his family, or the office which exhibits its array of surplus profits and a rapidly accumulating capital, the idea is monstrous that we should be called upon to give opinions of such importance, without fee or reward. How often has it occurred that one of us has saved the office, applying for our gratuitous opinion, a thousand or more pounds, and at the same time disobliged a valuable patient! No fee which the office can give us can adequately reward this service, nor, on the other hand, give us our share of the gain which accrues to the office when the report is favourable; but that is no reason why the principle should not be recognised and acted upon as far as is consistent; and had not the evil crept upon us by degrees, and, moreover, had not the great mass of the profession, the general practitioners, been left without any bond of union, it could never have continued to the present time. I will not, however, descant further upon the circumstance itself, nor upon that train of events which has led the public to set so light a value on our opinions, but proceed to inform you that there is now an exception to the general practice of offices in this respect,—the directors of that one to which I am attached,* and which is rapidly

* The *Star* Life-Assurance Society, 44, Moorgate-street, London.

rising in public estimation and importance, having lately come to the resolution that, in future, "every medical man to whom reference is made shall receive a fee for his opinion (whether favourable or not), such medical man being a legally-qualified practitioner."

I trust that this example will not long be solitary,† and that the older offices will see the propriety of joining in a measure which is obviously so just and proper. I am, Sir, yours obediently,

JOHN M. CAMPLIN.

Finsbury-square, Jan., 1844.

WESTMINSTER MEDICAL SOCIETY.

Saturday, Jan. 6, 1844.

Mr. FISHER, President.

ACUTE INFLAMMATION OF THE PROSTATE.—EFFECTS OF CUBEBS IN GONORRHŒA.

MR. ACTON said that the rare occurrence of acute inflammation of the prostate in the modern treatment of gonorrhœa deserved particularly the attention of surgeons; and he thought that the recital of a case now under his care might prove interesting to the society, as involving several practical suggestions, which he imagined had not been sufficiently dwelt upon, in consequence of such cases being identified with disease of the bladder and urethra:—A. B., a gentleman, twenty-eight years of age, of a delicate constitution, consulted Mr. Acton for a gonorrhœa of some few days duration. He complained of some scalding, but the inflammation in the urethra was by no means violent. The usual treatment with cubeb capsules, containing a solution of the active principles of cubebs in ether, was prescribed, and the case went on well for some days, when the patient complained that micturition was more frequent, and the urine passed in small quantity, accompanied with pain and scalding in the perineum; and these symptoms were not relieved by the frequent passing of water, but were followed by a desire to strain and make water after the last drops were voided. Going to stool was likewise attended with a sensation of scalding in the gut during defæcation, and the finger introduced per anum was attended with a great deal of pain and spasm of the sphincter, but it did not detect the increased heat of parts described by our author; the gland was found somewhat enlarged. In accordance with the treatment usually followed in such cases, warm baths, leeches in considerable numbers, and opiate enemata were tried, and found ineffectual; general bleeding was not resorted to, as little constitutional disturbance was present. Retention now came on, and no other alternative presented itself than to draw off the urine by means of the catheter. A silver instrument was used; its introduction was attended with a considerable degree of pain when passing over the inflamed gland, and it was necessary to depress the handle of the instrument to a very great extent, as in cases of chronic enlargement of the gland. A large quantity of highly-coloured urine was drawn off, which had been since analysed by Dr. Griffith, who had promised to furnish Mr. A. with the results. The patient was enabled to make water in small quantities during the night, but the retention gradually returned in spite of all the antiphlogistic means employed, and on the next day the urine was again drawn off, which was followed by the same results. Under the usual treatment the patient had now got nearly well. The points of practical interest to which Mr. A. begged to call the attention of the society were these, What share had cubebs in producing this disease, which was one of the severest complaints he had witnessed in the course of his practice? He recollected a case similar in its symptoms, though far less severe in degree, which occurred during the administration of cubeb-capsules some months previously; and a patient, who had been formerly under the care of Sir B. Brodie, had lately refused to take cubebs because that eminent surgeon had told him that cubebs was a very pernicious medicine, and laid the foundation for many diseases of the urinary organs. Mr. Acton wished to ask the opinion of the society on this point; he had given the medicine in much larger doses and in a large number of cases, and had witnessed M. Ricord's treatment, who is very fond of the remedy, yet, with the exception of these cases, he had never witnessed any bad effects. The other practical point to which he wished to direct the attention of the society was a recommendation to introduce the instrument at an earlier period than was usually recommended in cases of acute inflammation of the prostate. When that organ was acutely inflamed partial retention he believed occurred early; and this pressed on an acutely inflamed gland, which was not only itself swollen, and attended with a puffy state of the canal, but also blocked up by the tenacious ropy secretion described by Sir E. Home, as found in post-mortem appearances, and always to be met with in the water of these patients. Moreover, the slight chance of these symptoms being allayed suddenly, so as to allow of passing water, the constant efforts at micturition, which only aggravated the disease, without emptying the bladder, induced him to propose, as the least of two evils, to draw off the urine before perfect retention had taken place. The operation was a painful

† There is only one office which, so far as I am aware, professes to reward medical men; and that office does so only when their opinion is specially applied for, and not in ordinary cases.

one at the moment, but the relief was surprising, and in the intervals the disease became more amenable to treatment. On subsequent occasions passing the catheter was much less painful, which he attributed to drawing off the water before the retention had amounted to what it had been in the first instance.

Mr. H. J. JOHNSON related several cases, in which disease of the prostate had come on during the time the patients were taking cubebs; he did not, however, think that these ill consequences were confined to that medicine alone, as he had seen similar effects happen during any *repellant* treatment, and he confidently believed that Sir B. Brodie was much opposed to the use of any preparation of cubebs. He (Mr. H. J. Johnson) objected to this, the repellant treatment, when inflammation was present, and differed from Mr. Acton as to the necessity for giving cubebs where scalding was present; he thought it far more desirable to treat such cases by antiphlogistic measures than by the early administration of cubebs. In his experience of these cases he had found that the ordinary means, particularly cupping in the perineum and opiate enemata, were effectual, and he doubted if the early introduction of instruments was advisable. He believed that Sir B. Brodie's recommendation (mentioned by Mr. Acton), of passing a gum-elastic catheter without a stilet, applied especially to chronic enlargement, and not to the acute stages.

Mr. ACTON wished it to be understood that he had never advocated the repellant treatment in the inflammatory stages, as Mr. Johnson must be fully aware; and he was happy to say that cases similar to the present were of very infrequent occurrence. Did he agree that they were commonly met with, he would not have cited this one. As standing alone it tells most strongly against the repellant treatment, but viewed as an occurrence liable to happen it became interesting, more especially as we rarely met with so complete a case of inflammation in the gland. Mr. Acton, in conclusion, stated that in practice he did not find cases of gonorrhœa so rebellious as had been stated, provided common attention were paid to the treatment. He wished here to observe that properly using the injection he considered one of the principal causes of his success; he was in the habit of teaching each individual patient the means of bringing the injection in contact with every portion of the affected urethra. Without these precautions all other means must be futile, and it was to their application that he attributed his successfully curing gonorrhœa usually within a week.

Several members stated that they had given cubebs to a large extent, and that their use had not been followed by any unpleasant consequences.

Mr. CHOWNE related a case of obstinate gleet which had been cured under the direction of the late Mr. Tyrrell, by infusion of leeks.

Mr. SNOW said that Dr. Prout attributed many cases of urinary disease to the effects of medicines taken for the cure of gonorrhœa; it was, therefore, desirable to use as little as possible of cubebs and balsam of copaiba in the treatment of this disease, and more especially as it was one which had a strong tendency to get well of itself. He thought the most prudent plan was to be content with antiphlogistic and diluent treatment until all inflammatory symptoms had subsided; and then to use the balsamic remedies and mild astringent injections, if the complaint continued in a subacute form. There had been two persons poisoned in America not long ago, by taking cubebs in the dose of a tablespoonful from a certain parcel of this medicine, which circumstance ought to be borne in mind in prescribing it.

Dr. Griffith has favoured us with the following analysis:—

Urine very turbid, of a milky cast; on settling the upper part became clear; it was acid, filtered with difficulty, and contained a quantity of glairy, gelatinous mucus, but it was not dense; the sediment was not soluble by heat, except in very small portions (nitrate of ammonia). The constituents were otherwise normal save albumen, the presence of which was detected by the reactions of heat, nitric acid, and ferrocyanide of potassium after acetic acid.

The sediment contained a considerable number of pus and mucus-globules, giving the luminous rings and nuclei by acetic acid; but they did not form a separate purulent deposit.

DANGER OF ZINC IN BUILDINGS.—M. D'ARRET has recently shown (Annales d'Hygiène) that, in case of fire, the use of zinc as a covering to buildings is attended with considerable danger, from its combustibility. He has seen a stove-chimney made of this metal burn with frightful rapidity. In a memoir inserted in the "Records of the Academy of Sciences" for the year 1747, we find the following passage, which bears directly on the subject:—Formerly zinc was used to cover the towers of churches, but its use has been discontinued owing to its having been found dangerous in fires which took place in these churches.

DISEASE in the brain, spinal cord, heart, lungs, stomach, intestines, liver, kidneys, or other vital organs, is characterised more by disturbances of function in those organs than by pain.—*Dr. Bellingham.*

BRITISH MEDICAL JOURNALS.

THE January number of the "London and Edinburgh Monthly Journal" contains a paper of considerable interest, namely, one on the "STATISTICS OF FRACTURES," by Mr. LYON, late surgeon to the Glasgow Royal Infirmary, who, taking advantage of his opportunities, has described the result of two years experience in that institution. He alludes to the peculiar circumstances of those portions of the labouring and manufacturing population of Glasgow, which are particularly exposed, by their occupations, to severe and fatal injuries; and he mentions having had under treatment for fracture, during the above period, 196 individuals, presenting 202 cases of the above form of accident. Of the 196 cases, 163 were males, and 33 females. The following table shows the relative frequency with which fractures of the several bones occur, distinguishing, also, the sexes:—

	Males.	Females.
"Of Tibia	51	6
Fibula	45	8
Femur	33	8
Humerus	22	3
Radius	14	3
Ulna	10	2
Clavicle	8	3
Ribs	10	1
Scapula	4	2
Skull	4	1
Lower jaw	4	0
Bones of foot	4	0
Bones of hand	3	0
Spine	3	0
Of face, sup. maxilla, ossa nasi	1	0"

Our readers will remark the much greater frequency of fractures of the lower than of the upper extremity. We are led to infer, from the existence of so great a disproportion, that some injuries of the latter description, in consequence of their *less severe character*, were either not recorded at the hospital, or received relief from some other source. On considering the ages at which fractures occur, it is shown that in females, *as life advances*, the proportion of those injuries becomes greater than in males, a fact which is scarcely explained by "a greater proportion of the former reaching old age than the latter." We find, also, another more curious, however, than important fact, connected with the sexes, viz., as to the *side of the body* on which these injuries occur. There were in 153 males, 79 cases of fracture on the right side, and 74 on the left; whilst in 32 females there were only 10 cases of fracture on the right side, and 22 on the left.

Passing over some matter which is too copious for extract, we find some facts which are so entirely opposed to the generally-received opinions in surgery, that we shall specially notice them. They refer to the *season of the year* at which fractures are most frequent. Mr. Lyon remarks that—

"There is a prevalent opinion, that from the benumbing effect of cold, especially on the old, from the hardness of the ground during frost, and from the slipperiness occasioned by ice, and consequent frequency and severity of falls, fractures are more numerous during winter than other seasons. This opinion has been disproved by M. Malgaigne and others, and my tables corroborate their statements. To make this point evident, I have divided the year into three periods, and have affixed the number of fractures occurring during each, viz.—

	Cases.
"March, April, May, June	69
July, August, September, October	63 [62?]
November, December, January, February	55
Omissions	10
	197[196?]"

With regard to the *mortality* from fractures, we find 29 deaths to have taken place in 196 cases. Of these 12 occurred in cases of simple fracture, of which there were 152, and 17 in cases of compound fracture, of which there were 44. Of the 12 deaths in *simple fracture* there were "7 of the skull, trunk, and pelvis," 1 of the clavicle, complicated with cerebral injury, and 1 of the tibia, where the patient died from peritonitis, leaving thus, 3 deaths from simple fracture of the extremities, or a mortality of 2 per cent. Those 3 deaths seem to have occurred in old persons, and from bed-sores. In the cases of *compound fracture* the mortality presents a striking contrast, being 17 in 44, or about 40 per cent. We wish that the example of such interesting though simple facts, brought forward with such very little trouble, could induce other hospital surgeons to throw off the garb of idle indifference in which they have enveloped themselves, and give to the profession some of the experience which it has fallen to their lot to acquire.

A paper in the same journal, by Mr. T. W. KING, contains a case of "TRANSPOSITION OF THE AORTA AND PULMONARY ARTERY," &c, in a child who lived to the age of two years and nine months, presenting, from birth, some appearance of *cyanosis*. It seems that the aorta arose from what must be considered as the right side of the heart. We must infer this from the heading, for the obscure manner in which the malformation

is described leaves the fact a mystery. A communication existed between the auricles, and another between the ventricles, to attempt to explain the formation of which, to insist on the usefulness of warmth in the treatment of cyanosis, and to call attention to certain of the author's papers which "have been overlooked" by some "medical journalist" or other, appear to have been the chief object of the communication. A paper thus characterised may well be supposed to contain little that is useful or interesting.

The same journal contains two cases of "VESICO-VAGINAL FISTULÆ" successfully treated, by Dr. KEITH, of Aberdeen. Both of them originated from protracted labour. In the first case, after the malady had existed for seven years, the woman introduced into the opening a pint bottle-cork, which, having been at first very useful, after a few months accidentally slipped into the bladder. The cork, of course, coated with the salts of the urine, and, by a fortunate coincidence, *occupied the fistulous opening*, "acting as a bullet-valve," leading, in the end, almost to its closure. The calculus thus formed subsequently became a source of much irritation, and required removal. This was effected by the lithotrite, and a very small fistulous opening was perfectly closed by the actual cautery. The case suggested to Dr. Keith the advantage which might result from the treatment of vesico-vaginal fistula by means of a "bullet-valve," which he would form of a thin India-rubber bag, filled with mercury. This could be punctured, and easily removed when the desired object was accomplished. The suggestion deserves consideration. It bears some analogy to another method, which we know has been successful in more instances than one, viz., that of an India-rubber bag, placed in the vagina, dilated with air, and serving as a compress.

The second case is of less importance; it shows the utility of the *actual cautery*, and both cases illustrate the little suffering to which its application gives rise. Dr. Keith is an advocate for its application at short intervals, when the fistula is not a large one.

The "Dublin Medical Press" for Jan. 3, contains a lecture delivered by the late Mr. COLLES, on INFLAMMATION, and some of its consequences. The discourse is plain and practical, without speculative views as to the nature of inflammation; the lecturer gives a general outline of its characters, causes, effects, treatment, &c. The following passages, conveying a few useful hints, convey some idea of the manner in which the subject is treated. Speaking of *cold applications* in the treatment of inflammation, Mr. Colles remarks,—

"Cold water or medicated lotions are often extremely useful, particularly when there is a sensation of burning heat in the inflamed part, but their long continuance, where the inflammation is very high, is not free from danger; mortification has often been brought on by reducing its temperature too much, or too quickly, particularly in delicate constitutions; this caution is necessary to be remembered, as we have sometimes other objects in view in applying cold over an inflamed surface, as in strangulated hernia, besides the mere service it may render the inflammation; we must, therefore, watch carefully that our cooling process is not carried on too long. Objections have been started to applying cold to an inflamed part in the immediate neighbourhood of the great cavities, in the apprehension of repelling the inflammation to the interior. I think the fear is perfectly groundless; I have never seen a case which bore out such a conclusion, and if cold lotions were desirable, I should have as little objection to applying them over the thorax or abdomen as anywhere else."

The following memoranda on blood-letting are useful:—

"Recollect that by taking away the blood suddenly, and making the patient sit up in bed while bleeding him, so as to make him faint, you *save* blood, and consequently the patient's strength. Ten ounces drawn properly may have more real effect on the disease than five times the quantity without such precaution, and an actual saving of blood in some of these cases is very important. But take care that you do not mistake the cause of fainting—it must be from the detraction of blood, and not alone from the patient's timidity."

A rather doleful ditty occupies the pens of the editors in another part of the Dublin journal. We give it in the next page, as a curiosity in medical journalism, and for any benefit that its appearance in our columns can render to the journal. We have heard of an editor or two elsewhere who may appropriately parody the article.

PNEUMOTHORAX.— On this subject the "Medical Gazette" of this week contains a paper by Dr. HUGHES, of Guy's Hospital. We have always distinguished *hydrothorax* as a collection of fluid in the pleura, and *pneumothorax* as a collection of air in that cavity, and have recognised the term *hydropneumothorax* as implying the existence of both at the same time. Now, this distinction is lost sight of by the author, and the subject is thus rendered more obscure than it otherwise would have been. Considering the *causes* of [hydro] pneumothorax, it may arise from those acting externally to the lungs, or from lesions of those organs themselves:—

"Among the former set of causes may be mentioned,—

"1. The fluid of an empyema bursting into a bronchial tube, and the inhalation of

air through the opening by which the fluid escaped.

"2. Laceration of the pleura costalis, combined with emphysema of the cellular membrane, arising from injury to, or ulceration of, the larynx or trachea.

"3. A fistulous communication between some portion of the alimentary canal and the sac of the pleura, and perhaps—

"4. Gangrene of the pleura connected with necrosis of a rib.

"5. The decomposition of blood, and other albuminous fluids effused into the pleura, and,—

"6. The gaseous exhalation of the pleura itself."

To these we should add another source of simple pneumothorax, viz., an external wound, communicating with the pleural cavity, which is well known to be a not unfrequent cause of this affection, and which is here omitted:—

"The causes of [hydro] pneumothorax connected with lesion of the lung itself, are the following:—

"1st. The opening of a tubercular excavation into the pleura.

"2nd. The escape into the pleura of air through softened tubercular matter, communicating with a bronchial tube, but not connected with an excavation.

"3rd. Gangrene of the lung and pleura.

"4th. Laceration of the lung and pleura from external injury.

"5th. The bursting of an enlarged pulmonary vesicle from the violence of coughing, in emphysema of the lung."

The symptoms of the disease are then discussed without presenting anything that is very novel. There is much difference in the effects observed of the entrance of air into the pleural cavity; differences which, in Dr. Hughes' opinion, may be accounted for by a reference to the state of the surrounding lungs and of the passage by which the air has entered. He believes—

"That it will be generally found that, *cæteris paribus*, the less diseased the lung, and the smaller, within certain limits, the opening into the pleura, the greater will be the distress on the occurrence of pneumothorax; and conversely, the more diseased the lung, the greater the extent of pleuritic adhesions, and the larger the opening, the less will be the anxiety and collapse after that accident."

We may say of the paper, on the whole, that had it been less prosy it would have been more worthy of perusal.

The last number of the "Provincial Medical Journal" does not contain sufficient matter of interest to detain us from other labours this week.

COMPLAINT OF THE DUBLIN MEDICAL PRESS AGAINST ITS NON-SUBSCRIBERS.

We have now to tell the medical men of Ireland plainly, that if they want our support they must give us theirs. We cannot maintain an expensive journal for their benefit out of our own pockets, or labour like horses in their service without some evidence that the value of our exertions is appreciated. We, of course, direct our observations to those who, availing themselves of our exertions and funds, and of the exertions and funds of others on other occasions, are content to look on at a distance, watching what they can gain without loss to themselves. The plain state of the case is this. This journal was not undertaken as a mere commercial speculation, or with the hope of its ever affording its conductors adequate remuneration for the time, labour, and expense bestowed on it; but with the hope that it would at least produce a sufficient return to enable them to conduct it respectably, and with advantage to the subscribers. In this, we admit, we have not been disappointed; but we think it necessary to remind those who profess an anxiety to co-operate in common efforts for general good, that the sooner they give practical proof of their sincerity the better. We should often be amused, were we not as often irritated, by the frequent applications made to us for favours in our editorial capacity by persons who are not subscribers, as if we had nothing else to do but to provide for the wants of the selfish and greedy. To such persons we have now, once for all, to give notice that unless they are subscribers to the "Medical Press" they have no claims upon its conductors, or right to expect to share the advantages afforded in its columns. Some worthy gentlemen, we find, are in such a happy state of ignorance as to everything going forward in the medical world, that they are not ashamed to stop others to make inquiries respecting the most notorious occurrences of the day, and even pretend to be quite proud of the freedom they enjoy from the drudgery of all mental labour. When, to such inquiries, it is replied that they surely must have been all this time asleep, or they could not be unacquainted with what has so often been commented on in the "Medical Press," they rejoin that they really never read it, or anything of the kind, that they have not time, or they do not like its politics; in fact, they affect to boast that they are quite above such vulgar schoolboy practices as reading the nonsensical productions of these degenerate times, and that they are so entirely engrossed by the cares of practice that they have neither inclination nor leisure to think of anything else.

Now, we should be glad to know whether these gentlemen speak thus to their patients, when questioned, for the first time, as to their opinions respecting some novelty already made known to the public at large, although hidden from them. We know that they do not, but on the contrary are obliged to resort to all kind of evasions to escape exposure of their ignorance. If these gentlemen who "never read" subscribed to some other journal, or even purchased now and then a new publication, we should not have any right to complain, but upon inquiry in quarters necessarily well-informed on the subject, we find that the number of men pretending to practise medicine and surgery in Ireland, who never purchase either book or journal, is so great that we are ashamed, for the honour of our profession, to state it. We are assured that of the numerous candidates for "general practice" who blazon forth in golden letters on their windows the inviting titles of physician, surgeon, apothecary, and accoucheur, there are hundreds and hundreds who have not obtained a copy of a new edition of a dispensatory within the last ten years, and that of those who practise exclusively as physicians and surgeons, there are as many content to "rub on" with an old *vade-mecum*, or an ancient edition of the Surgical Dictionary. All this we are sorry to be obliged to state thus publicly, but we are not to conceal but to proclaim the truth as the only means extant for the correction of evils.—*Dub. Med. Press*, leading article, Jan. 3rd, 1844.

NEWS OF THE WEEK AND RECENT PUBLICATIONS.

Chemistry for Beginners. — A small volume of 208 pages has been published by Messrs. Longman, for Mr. Balmain, of Liverpool, designed to promote a knowledge of the fundamental principles of chemistry by presenting a "complete, but small, system of the science," written in the plainest terms which the author could use to express it. The principles being *theoretical*, and, therefore, not easily fixed in the mind, Mr. Balmain has divided his book into "Lessons," each lesson being devoted to one particular subject, in order that he might intersperse sets of questions at stated places through the work, by means of which the reader may every now and then *examine himself*, to ascertain if he has understood and recollected the preceding statements. Mr. Balmain considers that he has adopted the best order of topics for leading the learner gradually on, and we agree with him in that opinion. He has described no "experiments" in his lessons, believing that *beginners* can never perform them successfully; but he seems to contemplate adding, by and by, a supplement, devoted to chemical apparatus and its uses, for the hands of teachers who adopt his "Lessons,"—not a very likely adoption, however, amongst teachers who can think and arrange for themselves. We do not see that any particular *necessity* existed for this new work, the booksellers' shops already abounding with instruction of the kind. However, a *new* book will often produce a *new student*, and there cannot be too many temptations held forth to the world to study chemistry. The present volume is very well calculated to effect its object; we shall not, therefore, war with trifles in its execution. The exposition of facts in chemistry is perfectly clear, and conducted on the most modern theories.

What is the Immediate Cause of Insanity?—In a little book of about a hundred pages, published by Messrs. Longman, Mr. James Sheppard, Surgeon, of Stonehouse, discusses this question in an argument leading to an opinion, that the "proximate cause of insanity" is to be found in the blood. He considers his ideas to be "purely theoretical and speculative," but calculated, nevertheless, "to elicit practical information." He advises the medical officers of lunatic asylums to institute minute *chemical and microscopic* examinations; to ascertain, in cases of mental derangement, deviations of the blood from its natural standard; and he adduces "presumptive evidence" that insanity is always a consequence of its altered condition. His three texts are,—Can the mind act upon and alter the constitution of the blood? Can altered blood prevent the mind from being perfectly developed? Is the want of that development manifested by insanity? The volume of Mr. Sheppard forms a good introduction to a chemical course of inquiry into this disease. We strongly advocate attention to the subject.

Dr. J. R. Cormack has just issued, under the auspices of our worthy publisher, an account of the "Natural History, Pathology, and Treatment, of the Epidemic Fever at present (or lately) prevailing in Edinburgh." Several descriptions of this fever have already appeared in the journals, and such members of the profession as are further interested in the subject will find it treated at greater length in Dr. Cormack's work, with much ability and in an agreeable style. We shall probably notice the entire subject on a future occasion.

The deaths in the metropolis for the week ending Dec. 30, 1843, had increased over the week before by 39, the total having been 1007 (*vice* 968). The increase was chiefly in deaths from diseases of the brain, &c., 157 (*vice* 140 in the previous week), from

natural decay, 73 (*v.* 55), and from violence, 46 (*v.* 25). In deaths from diseases of the lungs, &c., there was a material decrease (290 *vice* 329). Deaths from diseases of the heart, &c., 24; of the kidneys, 9; in childbed, &c., 13; from organic diseases of uncertain seat, 109; from epidemic diseases, 196. Deaths from typhus had increased to 33 (from 25 in the preceding week), those from scarlatina to 54 (from 49), and from small-pox to 16 (from 11); those from measles had decreased to 27 (from 29), and from croup to 12 (from 15).

Dr. EDWARD WILLIAMS, M.B., Cantab, senior physician to the Essex and Colchester Hospital, has just published an essay, of 119 pages, on "The Tongue, in Functional Derangement of the Stomach and Bowels." We hardly need define more particularly than the quoting of the title defines, the contents of so brief a pamphlet. Suffice it to say that the work contains a series of observations on the tongue, more particularly its papillæ, in the diseases mentioned. In order, however, to deserve being depended on, the observations on that organ should be more extensive; while, to be *useful*, the conclusions should assume a more connected form, and be rendered more easy of application. The name of Messrs. Simpkin and Marshall is imprinted on the title-page.

ESSENTIAL PATHOLOGY OF CANCER.—From the researches of Müller it would appear that in the formation of carcinomatous structures "an arrest of development took place, and the primitive cells, instead of progressing in the construction of the more complex tissues of the body, took upon themselves the function of reproducing other cells, thus constituting, by their aggregate, a mass analogous to what we find forming the entire fabric of some of the lowest beings amongst plants and animals."—*Dr. Power, in Dublin Medical Press.*

APOTHECARIES' HALL, LONDON.

NAME of the gentleman who received a certificate of qualification to practise as an apothecary on Thursday, Jan. 4, 1844:—William Emmerson, South Shields.

CORRESPONDENTS.

We have received certain letters relating to the matters which have recently been discussed at Colchester, in which Dr. E. Edwards was concerned, but do not consider it to be necessary to quote from either of them any statements but the following, with which reference to *the facts* in print in this Journal had better close:—*Mr. A. T. Edwards* says, "In my note to Dr. Williams (LANCET, Dec. 30th) I read that there was *no* contract, but simply a verbal understanding. 2ndly. The papers, on the back of which Mr. Edwards's name and address were printed, were supplied by me, and used by Dr. Williams at my request. 3rdly. With respect to the 'vinum,' the form was well known, and frequently prepared by me, and, moreover, how could a form be 'private' which has been in print?" In another communication he adds that "Dr. Williams furnished him with the formula for the steel-wine; that he was in the habit of preparing the wine by the formula which Dr. W. gave him, from which the druggists which Dr. W. named were supplied; and that the Doctor stated to him, at the time, that the form was taken from a foreign Pharmacopœia." In a letter from Dr. Williams to the Editor occurs the subjoined paragraph:—"The third count of the Secretary's last letter contains an error or two, unintentional, of course, inasmuch as Dr. W.'s patient did *not* remonstrate—Mr. Edwards *possessed* the formula—Mr. Payne did *not* call on Dr. Williams." In concluding the correspondence we must observe, that the Society may reasonably be spared the trouble of vindicating its proceedings on this occasion.

The complaint of *Mr. O'Shea*, respecting the interference of Mr. Jones, relates to the case of a private patient. To notice it, therefore, in public, excepting to reply to a question on the subject, would be contrary to the custom of this Journal.

Samuel Pearson, who writes to us respecting the manner in which he was neglected in Guy's Hospital, should forward to us a description of his hand, written by the surgeon who is in attendance upon him. The conduct of the porter appears to have been very outrageous, but as it is already known to the steward, nothing would be gained by the publication of the charge against him. If it was illegal, a charge of assault might have been founded on it before a magistrate. It is impossible for us to judge correctly on a matter in which a statement is presented to us by only one of the parties concerned. Mr. Pearson evidently believes that he has been very ill used.

A Constant Reader.—The College of Surgeons required in 1830 two courses, of three months' each, or one of six months, of lectures on surgery; also hospital surgical practice twelve months, or during four years the surgical practice of a recognised provincial hospital, and six months, at least, of a London hospital.

ERRATA.—In Dr. Serny's paper in THE LANCET for Dec. 23, about the middle of the second column, on page 391, for "stages," read *strands* of the spinal cord.—In last week's number, in Mr. Philbrick's note, page 491, col. 2, line 16th from bottom, for "Leyden," read *Lexden*; page 492, line 5, for "I," read *One*.